

PRACTICAL OBSERVATIONS
ON THE
PATHOLOGY, PREVENTION, AND TREATMENT
OF
ASIATIC CHOLERA,
FROM THE AUTHOR'S EXPERIENCE
IN THE
EPIDEMICS OF 1849, 1853, AND 1866.
WITH
CASES.

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TO THE
Committee of the Cork Fever Hospital

THE FOLLOWING PAGES

ARE RESPECTFULLY DEDICATED

BY

THE AUTHOR.

P R E F A C E .

AT the request of several influential citizens I am induced to have reprinted from THE MEDICAL PRESS AND CIRCULAR, my observations on the Pathology, Prevention, and Treatment of Asiatic Cholera, together with cases treated in the Hospital, County of Cork Jail and Cork Fever Hospital, in the years 1849 and 1853, and in the latter Hospital cases treated *during the present visitation*; ~~also~~, the Committee having consented to afford such accommodation as they could, consistently with the more legitimate object of the Hospital, "the prevention and cure of fever."

This invaluable Institution, therefore, has been *the sole receptacle of Cholera* in this city and its vicinity since its outbreak, as it has at *all times* been the *sole protection to the Public* against all epidemics.

I am happy to be able to congratulate the public upon the fact that owing to the timely and efficient preparations made by the Committee and Medical Staff of the Fever Hospital, and the very energetic and landable exertions of the Mayor, the High Sheriff, and Sanitary Committee, together with the untiring and efficient services of the several Dispensary Medical Officers, the disease just now appears to have nearly subsided, a source of the greatest thankfulness to Providence when we consider how fearfully other cities have been devastated.

W. B.



PATHOLOGY, PREVENTION, AND TREATMENT
OF
ASIATIC CHOLERA ON THE OPIATE OR ANTIDOTAL
AND CONSERVATIVE PLAN.

As Asiatic Cholera has now come amongst us, and as I find that, notwithstanding the experience we have had, there are still so many conflicting opinions as to its pathology and treatment, and so many going over the beaten track, thereby losing not only time but life by persevering in remedies already tried and found ineffectual—one advocating the eliminating or purgative plan, another emetics, another astringents, another calomel, another the expectant or do-nothing plan, and another the mechanical plan, and none proving satisfactory—having had considerable experience in its treatment, and taken a particular interest in it, and much trouble in recording the results of my treatment in the years 1849 and 1853, when it prevailed here, and as one bedside fact is worth a thousand theoretical observations, and believing that etiological facts are only to be arrived at through *clinical experience*, I have resolved upon laying aside all private considerations, expressing my views of its nature and treatment, and publishing forty-five cases treated by me in the Cork Fever Hospital, and Hospital of County Cork Jail—thirty-seven on the opiate, or, as I think it might be called, the antidotal or conservative plan, and eight promiscuously, having previously tried all other methods in vain. Out of the thirty-seven I lost but eight (two in hopeless collapse when seen), and out of the eight I lost three (two under saline treatment). I may here mention that I was *driven*, I may say, to unusually large doses of opium in ordinary diarrhoea and dysentery previous to the outbreak of cholera, finding the usual astringents and ordinary doses perfectly ineffectual. When cholera, therefore, appeared, I was naturally led to test its efficacy in still larger doses.

The fact is, as long as we are at issue as to the *nature* of cholera, and therefore what the indications of cure are, so long must our treatment in this disease as in all others, prove, to say the least, experimental, empirical, and therefore unsatisfactory. The question is—Is cholera a blood poison, or a disease caused by prostration or suspension of the *vasi motor* powers? If a blood poison, why should it not be eliminated or carried off by the

kidneys as well as by the *bowels*? Why should nature select one and reject the other? We all know how often nature takes advantage of the *kidneys*, as a critical outlet in serious diseases.

With regard to the *exciting cause* of the disease, there are so many conflicting opinions, so many ingenious theories have been advanced, and so much written upon the subject, that time will not permit me to discuss the matter here. I shall, therefore, merely say that I think we may rationally suppose it to be caused by some peculiar atmospheric condition capable of unlimited diffusion, and rendered more active by local or predisposing causes, as miasmata, want of cleanliness and ventilation, overcrowding, &c. ;* but whatever may be the exciting cause, the brain and great nervous centres, the solar, gastric, and splanchnic plexuses, seem to me to be primarily and principally involved, so far as being deprived of their *tone* and *energy* (a remarkable peculiarity in cholera being retention of *consciousness* to the last), hence headache, vertigo, deafness, noise in ears, and a train of symptoms follow which constitute the disease. The nerves of circulation and respiration are also implicated.

It may be defined an exhausting disease, in which there is an *excessive elimination* of disintegrated blood, an inverted state of the system, what should pass off by skin and kidneys passing off by stomach and bowels. The indications or requirements of cure, therefore, appear to me to be—1stly, to restore the tone and energy of the brain and nervous system; 2ndly, to check all excretions and determination to internal surface (the cause of præcordial oppression, or, as the patient expresses it, “crushing of the heart,” so much complained of); and 3rdly, to promote capillary circulation and cuticular secretion, and counteract effects of vascular depletion.

The desideratum to baffle cholera, therefore, seems to be to discover some medicine that will *promptly* (FOR TIME IS EVERYTHING) meet these indications, and will be *soothing to the mucous membrane, stimulant to the nervous system, astringent, and non-eliminative*.

Opium, I assert, answers all these requirements of cure, if given *at once*, and in doses suitable to the *stage of the disease* (as quinine is given in ague), and in proportion to the *quantity, quality, and frequency* of the discharges, and amount of nervous prostration. I know no drug which exerts more, or even as much, influence over the nervous or sympathetic system as opium, and therefore (if my view is correct) *the only medicine* to be relied on in cholera, particularly if aided by the horizontal posture, which, for obvious reasons, must be rigidly enforced even in the premonitory diarrhoea; also by every means calculated to promote per-

* See an article by Dr. Holland, entitled, “On the Hypothesis of Insect Life as a Cause of Disease.”

spiration, so as to reverse the inverted state of the system, as external heat by hot solid substances—viz., bottles or jars of hot water, hot bricks, and hot sandbags placed in the axillæ and between the legs; also by hot mustard stupes to feet, and sinapisms over region of heart and pit of stomach. *The judicious and moderate* use of cordial stimulants spiced, and copious warm or cold diluents, as the patient may wish, as whey, toast water, mint tea, soda water, and *cold water ad libitum*, to which I find the addition of a little sweet spirit of nitre renders it not only grateful but useful in tending to skin and kidneys; drinks of the dilute nitromuriatic or sulphuric acids are also grateful and useful; the patient always craving for “something tart;” a little iced water, or a piece of ice put in the mouth, is also very palatable, and tends to allay the sensation of burning heat in the epigastrium, the incessant thirst and irritability of stomach. And here I would dwell especially upon the fact that nothing predisposes more to an attack of cholera than intemperate habits, and therefore, when the drunkard is attacked, his prospects of recovery are very bad indeed, as his stomach and nervous system have become already so accustomed to abuse of stimulants, that when paralyzed by the disease, difficult as it is to produce the desired effect upon a mucous membrane and nervous system in cholera not previously so abused, how much more so must it be in the case of the intemperate and the drunkard? Perspiration tends to excite the absorbent system, so that where this is the case less opium is required; also to counteract the possibility of any narcotic effect from the opium, which in itself produces a most extraordinary amount of perspiration, so much so that the nurse-tenders in charge of the cholera patients used to remark “that they were tired of mopping them.”*

Opium, then, besides allaying pain, promotes warmth, gives energy and tone to the capillary circulation, calms irritation of nervous system, and equalizes the balance of the circulation, and it is “conservative” in its effects, inasmuch as by controlling or checking the losses it sustains the strength, gives time for treatment, counteracts injury to the alimentary canal and shock to the nervous system from too rapid and profuse losses; in fact, it is the “anchor that holds on the barque of life.” As fullness in the blood-vessels of the brain is a sure preventive to the use of opium, so is an emptied state of the vessels (as in cholera) an indication for its use, and an anæmic or exsanguinated state of the vessels of the brain disarms the opium of its narcotic property.

The effect produced on the brain and nervous system in cholera is, in my opinion, in many respects similar to that caused by prostration of

* The sympathy between the internal and external surfaces of the body is very remarkable. The alternations of perspiration and diarrhœa that occur in the last stage of consumption afford an instance. When the diarrhœa is checked the perspirations increase, and when the perspirations are checked the diarrhœa is promoted.

nervous energy from sudden and excessive loss of blood, as in uterine hæmorrhage, where opium (as is well-known to the profession) is one of our most valuable medicines, and the quantity that can be borne without narcotism when the losses are excessive is incredible. In an excellent work lately published on Cholera by Dr. MacPherson, the case of "a sickly Indian native" is mentioned, who took 600 drops of laudanum in one night and recovered; and "a gentleman was saved from impending death by taking 400 drops; it never in the largest quantities produced affections of the head." Not only is it useful in vascular depletion, but in affections of the nervous system, as lock-jaw, where two ounces of the tincture is said to have been taken with advantage and without narcotism; also where pain is present, as in passing of renal calculi—in fact, in every affection where it meets a counteracting agent. It is, therefore, in cholera the antidote, and this is one reason why the doses I have given had no narcotic effect. In cholera, therefore, it is useful not only in checking excessive secretions and vascular depletion, but in counteracting great nervous depression, exciting the animal electricity, and by its stimulant properties correcting the anæmic state of the vessels of the brain, thereby maintaining the equilibrium of the cerebral circulation, and favouring a certain amount of congestion and consequent pressure necessary for healthy cerebral action.

A. T. Thompson says—"In all cases where there is a deficiency of blood in the capillaries, opium is to be preferred to the salts of morphia; its stimulant properties excite the capillaries, which relieves the internal congestion, and brings on sweating as a critical exertion. Combined with aromatics it increases its stimulant effect and lessens its sedative; it increases the energy of the brain and contracts the diameter of the vessels which include the excretory ducts through which the serum passes, and diminishes all the secretions and excretions, except the cuticular, which it promotes." Dr. Armstrong, in his work on "Scarlet Fever," says with regard to opium—"I cannot better illustrate the effects of this drug in cases where excessive irritation and debility exist without organic lesion than to compare it to the effect which it produces in the last stage of cholera morbus, sometimes snatching patients from the jaws of death. My general experience of the efficacy of this medicine in copious eruptions of blood, fully confirms the commendations which Dr. Stewart has bestowed upon it in uterine hæmorrhage, when the system has been excessively exhausted by loss of blood, or extreme nervous agitation arises. This agitation is often so surprisingly calmed by opium that I have seen patients seemingly in the jaws of death saved by its administration."

Dr. Hughes (Assistant-Physician to Guy's Hospital) says—"I consider cholera a huge drain from the alimentary canal, which, whether excreted from the anus or merely secreted into the intestines, I believe to be the

true cause as well of the collapse as of the diminution or cessation of the secretion of bile and urine. For stopping this drain I have found nothing so effectual as a large dose of solid opium by the mouth, followed by astringents of ammonia and opium in a fluid form, with an enema of starch and full dose of laudanum. If these means are employed early in cholera (and by cholera I mean neither diarrhœa, however profuse on the one hand, nor the collapse of cholera on the other), I believe this will be found generally effectual in checking the disease."

Mr. Brown trusted to opium alone in solid form; Mr. Delany, opium in fluid state (150 drops in brandy). Tweedie and Gassalie found opium more effectual than any other remedy. Orton and Sydenham "opium the sheet anchor, and one dose enough to cure the disease" (see Orton's Essay on Cholera in India). Welshman, opiates and diluents. Jobert, laudanum in seltzer water. Ryan, "full doses of opium to allay disturbance of nervous system, brandy and ammonia to support strength, and full doses of opium to arrest the discharges, the disease being thus easily cured." Tweedie says—"Cholera may be arrested at any period anterior to collapse by efficient doses of opium—the Chinese opium and camphor." Dr. Mahir employed in the Polish army large doses of opium and prussic acid. Dr. James Johnson lauds stimulants and astringents as severally well adapted to the stages of diarrhœa and collapse. "The fit (he says) is readily arrested by opiates, if recent, and the blood not too far deprived of its serous constituents."

I could quote many more eminent authorities in support of my views of the nature and treatment of cholera did time permit me, want of which, together with anxiety that the results of my observations and experience should be made public while the disease is amongst us, must plead my apology for the crude and imperfect manner in which they must therefore appear. In advocating the use of opium, and on reference to the cases I have treated, it will no doubt be remarked that the dose has, in some cases, far exceeded that usually given in the most obstinate cases of ordinary diarrhœa. The fact, however, of the total absence of the usual narcotic effect of a large dose of opium, except in one case (where twelve grains and a half were taken at one dose, with perfect recovery from cholera), proves, I think satisfactorily, the tolerance of this drug, especially when the same dose (short gr. ss.) was given in a similar case without the least inclination to sleep; on the contrary, the patient's remark was, when asked how she felt some hours after she had taken it, "I would be quite well now if I had a little sleep," and twenty-five drops of the tincture of opium now had the desired effect. This fact proves how absurd it would be to expect any benefit from such a dose as a grain of the powder or fifteen drops of the tincture in a case of cholera, as has been recommended. It should also

inspire confidence in those who believe opium to be the remedy (and I believe the majority of those who have had any experience in the disease are of this opinion), but are afraid to give a suitable or efficient dose. Much better not give it at all, as, under those circumstances, it is not only sure to fail, but to get a valuable remedy into disrepute. That ordinary diarrhœa doses will not answer in cholera we have no less eminent an authority than the late Dr. Graves, deservedly of world-wide reputation in the profession, who was also an advocate for arresting the discharges. In speaking of acet. of lead and opium, he says, "It had been used at Grange gorman Hospital, but in small doses, insufficient to produce decided effects. I believe I can fairly claim the merit of being the first to give it in large and effectual doses by the mouth. I need not say of what importance it is to check the discharges from the bowels and stop the vomiting. As long as these exhausting discharges continue, as long as the serum of the entire body continues to be drained off by the intestinal exhalants, what hope can we entertain? I have frequently given as much as forty grains of acet. of lead in twenty-four hours with great advantage and no bad consequences."

With regard to the narcotic effects of opium in cholera, it is necessary it should be borne in mind that patients dying in cholera generally die comatose, or apparently narcotized, and I have, therefore, known instances where those appearances were attributed to an over-dose of opium, where, in some cases, it had not been given at all, and in others where the dose taken could not by possibility have had the effect. The tolerance of the drug may be accounted for in three ways :—1st, by its narcotic properties being counteracted by meeting the poison in the system (by poison, I mean loss of the vital fluid)—in other words, by its being the antidote; 2nd, by the inactivity of the absorbents; and, 3rd, by the exhausted state of the nervous system, which is in itself an antidote to the effects of stimulants, and creates a tolerance of wine and opium, proving, therefore, the utter inutility, nay, serious consequences (as it is time lost) of prescribing even for ordinary diarrhœa (especially when cholera is epidemic) a smaller dose of the tincture than from twenty-five to forty minims for an adult, and for an actual case of Asiatic Cholera, with a pulse distinct, a smaller dose than three grains of the powder, with a drachm of the tincture or about six or seven grains; and in a more aggravated case, short of collapse, a larger dose again, say six grains and a drachm of the tincture, or about nine or ten grains. I have often checked a case of decided cholera in the earlier stage with a drachm of the tincture, or from sixty to one hundred drops. Where vomiting is not very frequent, I prefer giving the medicine in draught; but when it is constant, in pills, as you can see them if rejected, and repeat accordingly, whereas if the fluid is rejected, you are, of course, at sea as to how much is retained, and therefore how much

to repeat ; and I may here remark that, as in the most fatal cases vomiting does not often occur till near collapse, the treatment should not be the less active. Cramps are also often absent in the most fatal forms, and you may have a case of malignant Asiatic Cholera, without either purging or vomiting, "cholera sicca," the most rapidly fatal form, which Majendie describes as "commencing at death"—a proof, I think, of my view of the nature of the disease (nervous paralysis or prostration), as I have originally mentioned. These are, however, fortunately rare and exceptional cases in this country, especially without purging, and occur only, in my opinion, when the shock to the nervous ganglia has been so sudden and so great that there was no power or time for the system to rally, as in a case of death from a stroke of lightning, or sudden concussion from any cause acting on the great nervous centres. Rapidly fatal cases have been described in India where spasm had been the only symptom ; but on post-mortem examination the bowels were found distended with the characteristic fluids. Out of sixty cases described by Dr. Jackson, in his report of cholera in Paris in 1832, there were only two without vomiting and five without cramps, and in the "Medico-Chirurgical Transactions" for 1838, in twenty cases there were two without vomiting or cramps. The following is the form of pills and mixture I prescribe, called anti-cholera pills and mixture:—

R Pulv. opii, gr. xxiv.

— capsiei, gr. xij., ℥ ut ft., massa e. ext. gentianæ. q. s. et in pilulas, xij. divide. Signr., "anti-cholera pills," from one to five, for a dose for an adult.

R Tinet. opii, ℥i.

Ætheris chloricæ.

Spt. ammon. aromat. aa. ℥ss.

Mist. camph. ad. ℥viij. ℥, in partes octo divide.

Signr., "anti-cholera mixture," one part for a dose for an adult.

I have hitherto ordered camphor, in addition to the above pills ; but as my chief reliance is on the opium, as a small pill is so desirable, and as camphor adds so much to the bulk, I have omitted it, especially as I usually order the mixture with the pills, which contain some camphor. In the stage of actual collapse (hopeless collapse, as it is often not inaptly called), no drug will have any specific effect, and even if it were absorbed (so far as opium is concerned), it will not be so much indicated, as the losses it was meant to check will have subsided of themselves, the patient will have been drained ; hence the necessity of arresting the disease before the algid symptoms set in ; at same time I would by no means discard the use of opium, but feel my way with it in smaller doses,

repeated according to circumstances, to assist in stimulating the brain and nervous system, and bringing about reaction. We must here trust very much to the *vis medicatrix naturæ*, at same time do all in our power, as in the earlier stages, to promote perspiration by external heat, sinapism over pit of stomach and region of heart, and, if necessary, a small blister, and the surface afterwards sprinkled with morphia, also hot mustard stupes to feet, and give copious warm or cold drinks, judicious use of stimulants, and cold or iced water *ad lib.*, &c., as I have mentioned, for the earlier stage; and as absorption by the rectum, in this stage especially, is more likely than by the mouth, I would recommend starch and landanum enemata, if necessary, with the addition of acet. plumbi, also nutritive enemata.

In regulating the dose of opium for a case of Asiatic Cholera it may be some guide if we keep three objects in view—1st, prescribe a dose sufficient to check the discharges (if they exist); 2nd, add to this a dose sufficient to restore the brain and nervous system to their normal state; and, 3rd, add further to these such a portion as will be sufficient to counteract the depleted or emptied state of the vessels. This will account in some measure for the dose being necessarily larger than on ordinary occasions, and why a smaller dose will be sufficient in collapse, when the discharges will have ceased. The great secret I find is to hit off a sufficient dose at once, as no repetition will answer so well; and this is important also, as there is too often no time for repetition, and I don't see the use of ordering a medicine to be taken every quarter or half hour (as is so often done), when, in nine cases out of ten, the patient, if he lives to take a second dose, may be even in that short time in hopeless collapse. You must, therefore, hit off such a dose of such a medicine as will be likely to meet the indications in the shortest possible time. Give an ordinary or inefficient dose at first, and the patient will assuredly be lost. With regard to infants and children, opium must of course be given with the greatest caution. A case has just occurred in the Cork Fever Hospital: a boy, aged seven years, brought in with serous purging and vomiting, pulseless, no urine, sunk countenance, and choleraic voice; two and a half grains had the desired effect, and next day reaction set in with a good pulse, hot skin, and he passed urine twenty-four hours after admission. Every powerful agent requires limitation and control, and nothing can be termed a remedy that is not used under appropriate circumstances, nor is any treatment deserving of confidence that is not founded upon the acknowledged and time-honoured principles of medical science, nor can any medicine be given at all times innocuously. It is incumbent on me here to guard against the idea that a large dose constitutes the treatment; on the contrary, it will be perceived by the cases I have treated that the dose varied

from two or three grains to ten or twelve (the average I found to be about six grains); it is no "rule of thumb" matter; it requires great discrimination, judgment, and experience, and the dose must be regulated and graduated, as I have said before, by the stage of the disease, the degree of nervous prostration, and the character and amount of the losses; extreme cases will require extreme doses, and a dose that would be not only proper, but necessary, when the blood-vessels have been drained by serous losses, would be not only improper, but highly dangerous, in the premonitory or early stage, before any escape of serous fluid has taken place. But what I wish to insist on, as the result of my observation and experience, is that opium, physiologically and pathologically speaking, is the remedy, but that success does not depend on it alone, but on the combined effects of the horizontal posture, external heat by solid substances, and every means calculated to produce perspiration; and I am confident that if every medical man carried in his pocket a small bottle of laudanum and pills, such as I have mentioned, and administered on the spot and before sending to hospital, not only would time (all important) be saved, but the disease (to say the least) would, in my opinion, in nine cases out of ten, be greatly, if not altogether checked, and the patient, by the time of arrival at hospital, be most probably in a fair way of recovery, and this I often found to be the case. It should always be borne in mind that the great pathological difference between cholera and other diseases is the rapidity with which the morbid changes take place.

In the cases I have treated on the opiate plan, the absence of consecutive fever is remarkable, and may, I think, be accounted for by the checking of the discharges.

With regard to "elimination," or assisting nature, as it is called, founded on this occasion, I take for granted, on the very erroneous homœopathic principle of *similia similibus curantur*. I am on all occasions a great advocate for assisting nature, and think that the best way to do so is to use such a remedy as will produce such a state of things as nature would present when in health, and this view of assisting nature will, I think, bear out my treatment, as, when the efforts of nature are excessive, and therefore injurious to health—nay, too often in cholera causing death—they must of course be controlled; and here, as in my opinion the use of purgatives in cholera (if generally adopted) would be attended with serious results, I must say a few words:

If the diarrhœa is of a bilious character and attended with pain, and that there is a good pulse and foul tongue, and reason to suspect that it is caused by some offending matter keeping up irritation, and should therefore come off, I would eliminate, so far as one dose of castor oil, or in preference some mild warm aperient, and even so, I would add a few drops of

landanum, so cautious would I be, during a cholera epidemic, of purgatives, especially saline or drastic. Dr. McIntosh says "he has known many destroyed by taking a laxative or emetic, and others fall into a state of collapse, while using saline medicines during prevalence of cholera;" and Dr. Laycock, in the *Medical Gazette*, says, "For my own part, I have such a dread of purgatives in Asiatic Cholera, that even after the patient is recovered I would allow two or perhaps three days to pass before prescribing even a gentle aperient, for fear of relapse." A case in point has just occurred to myself in hospital: a patient who had recovered from cholera, in whom the serious losses from the bowels had been, as he said, "so frequent he could not count them, before admission;" he got well, and was four days without a motion, when I ventured to give him a mild and warm aperient; the result was, it produced a return of the symptoms, and I had to treat him again in a modified form. He is now well. This I consider an important case, showing how cautious we should be in acting on a mucous surface, already over-acted on, even by gentle means, and in future I would prefer a simple warm-water enema. In a late number of the "*Medical Times and Gazette*" I find observations so much in accordance with my views on the subject, that I think I cannot do better than quote them.

Speaking of the "efforts of nature," it says,—“Let us see how nature deals best with poison, and how experience teaches us to treat cases in which the ‘efforts of nature’ may be either inadequate or excessive.

“Let us take the case first of a mineral or vegetable poison—say calomel, arsenic, or elatesium. Either of these substances, in certain quantity, sets up vomiting and purging, by which after a time ‘nature eliminates the poison,’ and the patient recovers; but let us suppose the dose very large, so that it gets, as we may believe, into the blood, that the vomiting and purging are intense and exhausting, does the physician aim at elimination, pure and simple? Not a bit of it; he seeks to put the patient into a state that shall render him less sensitive to the effects of the poison. He diminishes and controls the efforts of nature, so that a large dose of poison may act like a small one, and on a weak system like a strong one. The stronger the system the less violently does nature react against the poison; and that which will make a weak system act like a strong one is opium. Give repeated doses of calomel till they purge, producing perhaps intense tenesmus and bloody stools, and what is the treatment? Opium. Under the influence of this the poison is no more heard of; pain and discharges cease, and we may suppose that the poison eliminates itself quickly without damaging the alimentary canal.

“Let us take the case of poisons of a zymotic order—typhoid or cholera. If a moderate dose be administered to a patient in first-rate health and

spirits, how does 'nature' act? Why, she may not condescend to notice it. It is on patients exhausted, ill-fed, or already prone to illness, that the poison acts as a specific poison. To produce its full effect, it must provoke certain reactions in the system of the recipient; it must feed on the patient and multiply itself, and it is the weak, ill-fed, nervous, irritable, and exhausted who fall into those reactions and permit that multiplication most readily.

"Suppose, then, a population, breathing air, drinking water, swallowing dust, eating food with unwashed hands, all impregnated with cholera poison, and suffering from incipient bowel disorder. What is the indication? Is it to eliminate? Certainly not, but to recruit the forces, and to resist the action of the poison in the alimentary canal, and for fulfilling these indications the experience of half the world points to opium."

Dr. McCormac, writing in a late number of THE MEDICAL PRESS AND CIRCULAR, relates the circumstance of a house-to-house visitation in an Irish town. "Do you purge?" was the question asked, and if the answer was "yes," an opium pill was put into the respondent's throat "sans ceremonie." The result was successful. The system at large, and the alimentary canal in particular, were soothed, comforted, and rested; the poison was "locked up," but could do no harm; the patient was protected. And this, concurrently with the experience of hundreds of practitioners in the last three cholera epidemics, shows that soothing, astringent, and non-eliminative treatment of diarrhœa is a pretty good safeguard against the fully-developed phenomena of cholera.

With regard to "calomel," as it is still spoken of and prescribed, I must offer a few observations; and, first, I do not see how it is indicated, inasmuch as the hepatic function is not suspended. Though bile does not appear in the discharges, the liver is acting, and hence the distended gall bladder, always seen on post-mortem examination;* the secretion is retained, not suppressed; and the suppression is the effect, not the cause of the disease. We must first, then, cure the cause. Besides, calomel, though said to be a sedative in $\frac{1}{2}$ j. doses, is more or less an irritant, and those who advocate its use generally give it in small doses—they certainly steal in a little of the opium with it. Even so, calomel, in my opinion, only tends still further to promote a secretion from a membrane already too profuse.

Again, if it is "the specific action" is looked for, we all know how difficult it is to produce that, even when the absorbents are active, in a

* Some pathologists have noticed in such cases a stricture at the mouth of the "ductus communis choleddicus," preventing the flow of bile into the intestine, when pressure is made on the gall bladder.

shorter time than from twenty to forty hours. I find, on reference to the "Fever Hospital Journal," that in the year 1849 a patient recovering from pneumonia, and while under the specific influence of calomel, was attacked, notwithstanding, with cholera; so much for its preventive or curative properties in this disease. Mr. Orton, in his essay upon "Epidemic Cholera in India," says—"Calomel was frequently found at the bottom of the fluid contents of the stomach, and adhering to the mucous membrane." Except in the consecutive fever, then, or in convalescence, I do not see how it is indicated; besides, it will not act until the morbid action is arrested by other means, or by the "*vis medicatrix naturæ*." I shall say nothing upon the application of "ice to the spine," strychnine, &c., &c., further than that I trust whatever experiments are tried they may be founded on something rational. Stimulating liniments or dry mustard rubbed into the spine may be useful.

A few words on Prophylaxis or preventive measures: As nothing predisposes more to an attack of cholera especially than fear (the disease being in my opinion of the nature I have stated), it is most important that the public mind should be impressed with the fact that it is within the reach of the profession when promptly and efficiently treated that no disease requires less medicine, the one dose being generally sufficient, and that there need not be so much apprehension in attending upon their sick friends on the score of contagion, as in my opinion, generally speaking, it is only contagious when predisposed by fear, or from overcrowding and consequent want of proper air and ventilation, or from some depressing or debilitating cause; at all events, it is not disseminated, as contagious diseases, usually are, under circumstances of free intercourse. There are many proofs of its non-contagious character, and those in support of the opposite view are far from being decisive of the question. Time does not permit me to enter fully upon them. I shall merely mention the inefficiency of quarantine regulations in preventing its extension, the extraordinary immunity from the disease of the nurses and medical attendants in constant contact with it, under the most unfavourable circumstances, compared with that of other contagious diseases, and the following instance by the late Dr. Mackintosh (Edin.) in 1834. He says:—"In the Drummond-street Cholera Hospital (of which he was physician) there were 280 bodies examined. Two, and sometimes three, hours were spent in examining each body. The room was a miserable place, eight feet square; generally six or eight persons were present, sometimes more; and in an inner apartment, about ten feet square, there generally lay six dead bodies. Not one of those who frequented this den of death, and had their hands imbrued in the secretions of the dead for six hours out of the twenty-four, were affected with cholera, though their hands were irritated and punctured

daily.”* The shortness of the duration of the epidemic is, I think another proof of its non-contagious nature, and the remarkable immunity of the rich compared with the poor. As preventive means, maintain the tone of the constitution and nervous system as much as possible; avoid everything tending to irritate the digestive system, particularly the abuse of alcoholic stimulants; observe regular hours, and avoid everything tending to fatigue and debilitate the nervous system, and exposure to cold or damp. Therefore, wear warm clothing, and a light flannel or silk girdle round the abdomen, the great and rational object being to preserve the capillary circulation, particularly in this quarter, and thereby prevent reëeding of cuticular excretion to internal parts, and consequent congestion, and next in importance is the warmth of the feet and extremities, which, with suitable clothing, will be promoted by maintaining the circulation in the vital organs, and which, if not preserved, the remote parts supplied by those organs must naturally suffer.† Silk inside-clothing, as being a non-conductor of electricity, is very valuable, this essential to health being so deficient in cholera. As perspiration is the grand object to be attained in the cure of cholera, so is the preservation of the capillary circulation as a preventive to be observed.

Diarrhœa (even of a bilious character), if excessive, should be judiciously checked; as, during a cholera epidemic, I have known it when allowed to continue soon run into serous cholera.

The necessity for cleanliness and daily use of sponge bath, proper ventilation, and above all things avoiding overcrowding, and the use of pure water, is so obvious and so universally dwelt on, I need only mention the fact; also the free use of disinfectants, added to the excreta, and sprinkled about the rooms—as Condyl’s Fluid, Carbolic Acid, McDougal’s Disinfecting Powder, Chloride of Lime, Chloride of Zinc, &c. To insure pure water, boil and let it cool for use. Should any object (as of course they will) to these my views as to the nature of Asiatic Cholera, the indications of cure or treatment, all I can say is, that both one and the other are the result of personal bedside observation, experience, and no small amount of labour; and that I am not therefore prejudiced in favour of my pathology or treatment of the disease, on any theoretical or fanciful grounds. On the contrary, should any more rational views of its nature and more successful mode of treatment in the same number of cases be shown me, I shall gladly avail myself of both one and the other. As it is, I have the gratification to feel that I have, under God, been enabled to save many lives,

* *Prac. of Physic*, p. 345.

† The familiar anecdote of the man who was lost in snow, and whose life was preserved by his dog lying instinctively on his chest, and thus keeping up a degree of heat over the region of the heart affords an interesting example of the importance of this observation.

which, judging from past events, would have been sacrificed by other or more feeble treatment.

“Whoe’er thinks a faultless piece to see,
Thinks what ne’er was, nor is, nor e’er shall be.”

In 1849, having published my treatment, I received a letter from a lady in Castlemartyr thanking me and begging of me “to continue to send help far and near,” and stating that she had made inquiries of a clergyman in Belfast (where the cholera then raged) as to the result of the opiate treatment then adopted there, and that his reply was, that “having been one of the visiting committee of the General Hospital, sometimes passing whole nights there, in consequence of the conduct of the nurses to the patients, he had ample opportunities of judging of the treatment, and that he considered it eminently successful.”

I cannot close these observations without recording a case of extremely malignant Asiatic Cholera (in addition to the two already alluded to), just now convalescent in Cork Fever Hospital:—

October 25th, 1866, half-past nine o’clock P.M. Patrick Barry, æt. 15, paralysed from birth at one side; serous purging and vomiting incessant; no pulse; surface livid; eyes sunk; no action from kidneys since the day before admission; vox cholericæ. Habeat pil. opii 2 (gr. iv.); external heat, spiced drinks, &c.; mustard stupes, &c.; cold water *ad lib.*

26th, half-past eight o’clock A.M. No discharge from bowels; vomiting still incessant; no pulse; no urine. R. pulv. opii, gr. ij.; sinapism epigastrio.

Two o’clock P.M. No reaction; no purging; vomiting continues. Habeat mist. anti-cholericæ, ℥ss. (trœ opii, ℥ss.)

Five o’clock P.M. No pulse; vomiting quarts of serum on getting a teaspoonful of any fluid.

Half-past ten P.M. No discharge; skin icy cold; no pulse.

27th, nine o’clock A.M. No stool; vomited once since last visit; no pulse, but surface a little warmer. Habeat trœ opii, ℥xx.

Eleven o’clock A.M. Pulse distinct at 112, and action of heart distinct; surface warm and perspiring; slept a little; no vomiting.

Five o’clock P.M. Pulse 100; skin warm; bears everything on stomach.

Half-past ten P.M. Pulse 100; no discharge.

28th, nine o’clock A.M. Pulse 100, and fair strength; no action of kidneys yet; vomited once; no stool.

R. Spt. æth. nit.

Sp. vin. gal. aa. ℥j.

Aquæ, ℥iv.

Rt. Mistura.

Two o'clock P.M. No discharge; pulse 100; skin warm; tongue a little parched; a little fulness over pubis; catheter introduced and brought off a pint of urine (four days suppressed); gin, two ounces.

29th, eight o'clock A.M. No vomiting or purging; passed a quantity of urine; pulse 88, and strong; surface and countenance natural; convalescent.

When the primary symptoms in all diseases are made the most important, we shall then learn the advantage of promptitude and efficient treatment, and the danger of delay and trifling practice, especially in Asiatic Cholera.

If a powerful impression be not made at once, little good can be expected, so quickly does the stage of collapse set in; and it is to this fact being lost sight of—this golden opportunity lost—that the fatality may be attributed, rather than to its so-called incurable nature.

In support of my views as to the pathology and treatment of this disease, I beg to append the cases alluded to, and in addition to those already published, five more cases treated in the Cork Fever Hospital during the present visitation:—

John McAuliffe, æt. 50. Premonitory diarrhœa and vomiting for twelve hours before admission to hospital, both serous; no action of kidneys for two days; vox cholericæ; “breath,” he says, “going through his ears;” breathing slow and laboured, and countenance sunk; no pulse; tongue cold; fingers and lower extremities blue and corrugated; some heat of surface. Treatment—opiate (gr. vj.), ext. heat, &c.; in six hours after pulse distinct (96); copious perspirations; some natural sleep; no discharges; no urine.

Eleven o'clock, P.M. Spasmodic vomiting and hiccup.

R Tinct. opii, gtt. xv.

Acid hydrocyanic (Scheele) M. iv.

Spt. ammon. aromat., ℥ss.

Aquæ, ℥j. ft. Haust. 4ta qq. Hora Sûmendis.

Took three draughts; kidneys acted on fourth day; recovered; no fever.

John Collins, æt. 35. Premonitory diarrhœa and vomiting (greenish water) for five days before admission; kidneys acting; voice tolerable; respiration and countenance tolerably natural; skin cold but not livid. Treatment—Pulv. opii, gr. iij. Recovery in two days; no fever.

Johanna Hackett, æt. 44. Hopeless on admission. Premonitory diarrhœa and vomiting for forty-eight hours before admission (serous); so frequent, could not say how often; no action of kidneys for twelve hours; voice inaudible; respiration laboured and slow; no pulse; tongue

cold; eyes sunk; skin cold and livid. Treatment—Haust. tinct. opii, \mathfrak{Z} j., ext. heat, sinapisms, &c. In two hours and a half after, pulse distinct in both wrists; surface warm; no discharges; but sunk in twelve hours after, sensible to the last.

William Leader, æt. 24. Premonitory diarrhœa and vomiting (serous and solid curds) for twenty-four hours before admission; kidneys acting; voice feeble; pulse 60 and feeble; tongue foul, and abdomen full; skin rather warm.

R. Ol. Ricini, \mathfrak{Z} j. c. trœ opii, gtt. xxv.; stupes, &c.

Serous vomiting continued in great quantity; got pil opii No. 2, and Haust. tinct. of opii, \mathfrak{Z} j. (gr. vj.). Next day well; no fever.

Catherine Hogan, æt. 60. Premonitory diarrhœa and vomiting twelve hours before admission very frequent (serous); kidneys acting; voice feeble; pulse distinct; surface cold; got pil opii No. 2, and Haust. tinct. opii, \mathfrak{Z} j. (gr. vj.). Recovery on second day; no fever.

Bess Horrogan, æt. 40. Had been in hospital; recovering from dysentery and ran into cholera. Discharges like barley water; vomiting, character of first serous, second pure green, third brick dust. No action of kidneys for two days before admission; voice feeble; respiration oppressed and anxious; countenance sunk; pulse very thready; tongue cold; eyes sunk; skin inclined to be cold and bluish; says, "her voice is going through her ears." Treatment—Opiate, dose, gr. x., external heat and stimulants, &c. Profuse perspiration shortly after; discharges checked; pulse 120, and strong; kidneys acted on fourth day; next day required enema amyli, c. t. opii, \mathfrak{Z} j. Recovery fourth day; no fever.

Johanna Murphy, [æt.] 28. Diarrhœa and vomiting for one day (wheyish); no action from kidneys for eight hours; voice almost inaudible; respiration laboured, slow, and spasmodic; countenance sunk; pulse scarcely perceptible; tongue cold; skin cold, clammy, and inclined to be livid; cramps very troublesome. Treatment—Opiate, dose, gr. xij.; towards night complained of "want of sleep," and got gr. xxv. trœ opii, which had the desired effect. Kidneys acted on fourth day. Recovered; no narcotism; no fever.

A. Thornhill, æt. 20. Diarrhœa and vomiting for four or five hours (rice water); no action from kidneys, for how long could not ascertain; voice whispering; respiration low and sighing; pulse scarcely perceptible; tongue cold; eyes sunk; skin cold and clammy; cramps very troublesome; kidneys acted three days after admission. Treatment—Opiate, dose gr. viij.; in six hours after required enema amyli, c. trœ.

opii, ℥j., aect. plumbi, gr. x.; in eight hours after required haust. opii, ℥. in brandy and water. Recovered on fourth day; no fever.

Daniel Sheehan, æt. 22. Diarrhœa and vomiting (rice-water) about three days; no action of kidneys for forty-eight hours before admission; voice very low; respiration very slow; countenance sunk; pulse scarcely perceptible; tongue cold; eyes sunk; skin cold, clammy, and livid; cramps. Treatment—Opiate, dose gr. xij. No symptom of cholera after, but was under influence of opium, recovery on third day; kidneys acted on third day.

Edward Roche, æt. 22. Diarrhœa and vomiting for four hours (rice-water); kidneys acting; voice inaudible; respiration slow and laboured; pulse scarcely perceptible; tongue cold; eyes sunk; skin cold, clammy, and livid; cramps. Treatment—Opiate, dose gr. vj. Recovered; no fever.

James Regan, æt. 26. Diarrhœa and vomiting for an hour (rice-water); no action of kidneys for twelve hours; voice feeble; respiration remarkably slow; pulse fifty-two, and very thready; tongue cold; skin cold and clammy. Treatment—Opiate, dose gr. viij. Recovery; no fever.

Mary Regan, æt. 37. Diarrhœa and vomiting for about eight days (rice water), since admission very frequent; no action of kidneys for twelve hours; voice almost inaudible; respiration very slow; countenance sunk; pulse very thready; tongue cold; skin cold, clammy, and bluish; no cramps. Treatment—Opiate, dose gr. vj. Reaction in two hours; recovery in three days; no fever.

M. Lehane, æt. 35. Diarrhœa and vomiting (serous) for a week, more or less; no action of kidneys for nine hours; voice feeble; respiration slow; countenance sunk; pulse very thready; tongue cold; skin cold, clammy, and blue; no cramps. Treatment—Opiate, dose gr. vj., and gr. ij. the day after. Recovered; no fever; kidneys acted on second day.

Pat. Kinnealy, æt. 12. Diarrhœa and vomiting (serous) for twelve hours; no action of kidneys for forty-eight hours; voice inaudible; pulse indistinct; skin cold and clammy; no cramps. Treatment—Enema amyli c., t. opii ℥., external heat, &c. Death in four days.

Eliza White, æt. 17. Diarrhœa and vomiting (rice-water) for about five days; no action from kidneys for twenty-four hours; voice hoarse and feeble; respiration and countenance natural; pulse very feeble; skin cold. Treatment—Opiate, dose gr. iiss. Recovery on second day; no fever.

Denis Mullan, æt. 40. Diarrhœa and vomiting for six hours before admission (serous); no action from kidneys for four days; voice very

hoarse and feeble; respiration slow and sighing; great noise in ears, pulse very thready; tongue cold and breath cold; skin cold and clammy; face and hands blue. Treatment—Opiate, dose gr. viij. Purging checked; recovery on fourth day; no fever.

Mary Buckley, æt. 20. Diarrhœa and vomiting most of the night (rice-water); action of kidneys could not be ascertained; voice very feeble; respiration slow; cramps very bad; pulse scarcely perceptible; tongue cold; skin cold, blue, and clammy. Treatment—Opiate, gr. vj. at once, and gr. ij. at night. Died within twenty-four hours.

Bridget Reilly. Diarrhœa and vomiting for twenty-four hours (serous); no action of kidneys for two days; voice feeble; respiration slow; countenance sunk; pulse perceptible; tongue natural; skin warm, but lower extremities very cold and livid. Treatment—Opiate, dose gr. vj. Reaction in about three hours; recovery on third day; no fever.

Cath. Regan. Diarrhœa and vomiting three or four days—the former gruelly; the latter, first serous, secondly sea-green water; no action from kidneys for twenty-four hours; voice feeble; respiration and countenance natural; pulse very feeble; skin warm, but face cold and clammy. Treatment—Opiate, first dose, gr. iiiss.; day after, R hyd. sub. gr. v., pulv. opii, gr. j.f t. pil. Recovery on third day; no fever.

Joha. Brady, æt. 30. Hopeless in fever when cholera set in. Diarrhœa every minute (serous); no vomiting; no action of kidneys for forty-eight hours; voice whispering; respiration very slow; countenance sunk and livid; pulse scarcely perceptible; tongue cold; skin cold, clammy, and blue; no cramps. Treatment—Opiate, dose gr. ij., external heat, and stimulants. Death in four days; no reaction all through.

Pat. Ahern, æt. 24. Diarrhœa and vomiting (serous) for some hours before admission; kidneys acting; voice feeble; respiration oppressed; pulse distinct, but very feeble; respiration laboured; pulse feeble; tongue natural; skin cold and livid. Treatment—Tinct. opii, ℥j., external heat, &c. Well in six hours; no fever.

Mary Cotter, æt. 31. Diarrhœa and vomiting (serous) for twelve hours; kidneys acting; voice feeble; tongue cold; skin cold and livid; noise in ears; cramps. Treatment—Trœ. opii ℥j. Discharges ceased; pulse 80, and full; profuse perspiration; recovering; no fever.

Portuguese sailor, dying on admission. Diarrhœa and vomiting (frequent and serous) for eighteen hours before admission; no action from kidneys, could not say for how long; voice feeble; respiration slow; no pulse; tongue cold; countenance sunk; skin cold, blue, cramps and hiccup. Treatment—Pil. opii No. 2, and haust. opii, ℥j.; in some hours after external heat, &c. Died in eighteen hours.

Bridget Leahy, æt. 30. Diarrhœa and vomiting for two days—the

first blackish water; second gruelly matter; no action from kidneys for four days; voice feeble; pulse 70 and feeble; thinks "something has got into her ears"; body and extremities warm, but face cold and clammy. Treatment—First dose, pulv. opii gr. v.; second, gr. iiss., and starch enemas. Recovery on fourth day; no fever.

— Burke, debtor. Diarrhœa and vomiting (purely serous) for eight hours; no action of kidneys; voice almost inaudible; respiration very slow; countenance sunk; pulse scarcely to be felt; tongue cold; skin cold, blue, and clammy. Treatment—Opiate, dose gr. iv., external heat. Death in six hours.

Mr. Sherlock, æt. 4 years. Diarrhœa and vomiting (rice-water); no action of kidneys for twenty-four hours; voice sharp and feeble; countenance sunk; pulse perceptible, but very feeble; tongue cold; skin cold and clammy. Treatment—Opiate, dose trœ, opii gts. xv., enema amyli, e, trœ opii gts. x., wine, whey, &c. Recovery on second day.

Mary Sullivan, æt. 48. Premonitory diarrhœa and vomiting for two days before admission (serous) incessant; none after, being drained; no action of kidneys for two days; voice inaudible; respiration laboured; great præcordial oppression; no pulse; tongue icy cold; countenance sunk; skin cold and shrivelled. Treatment—Pil. opii No. 2 (gr. ij.), external heat, stimulants, &c.

In four hours after temperature of body improved, but no pulse; slight return of serous purging; R̄ haust. tinet. opii, ʒij. Death in twelve hours.

Julia Reardon, æt. 60. Premonitory diarrhœa and vomiting (serous) about a week before admission; no action of kidneys for three days; voice, respiration, and countenance choleraic; pulse very feeble; skin cold; cramps. Treatment—Opiate, dose gr. vj., external heat, &c.; discharges checked and more natural; surface warm and perspiring; six hours after enema amyli. e. tinet. opii, ʒj. Recovered; no fever.

Bridget Leahy, æt. 30. Diarrhœa and vomiting (the former gruelly, the latter serous) for two days before admission; no action of kidneys for three days; voice feeble; respiration natural; pulse very feeble, 100, and intermittent; tongue natural; noise in ears; skin inclined to be cold. Treatment—Opiate, dose gr. vj., external heat, sinapisms, &c.; discharges ceased; next day abdomen distended with flatus; got enema terebinth. e. tinet. assafoetida; bowels moved so often got pulv. opii gr. ii. Recovered; no fever. This woman had cholera three weeks before.

T. Key Rogau, æt. 50. Diarrhœa and vomiting (serous) for six hours; no action of kidneys for forty-eight hours; voice inaudible; respiration very slow; countenance sunk; pulse scarcely perceptible; tongue cold; noise in ears; skin cold, blue, and clammy, and cor-

rugated. Treatment—Opiate, dose gr. vj., external heat, &c. Death in nineteen hours; no reaction from beginning.

T. Murphy, æt. 45. Diarrhœa and vomiting (serous) six hours before admission; no action of kidneys for forty-eight hours; voice inaudible; respiration very slow; countenance sunk; pulse scarcely perceptible; tongue cold; noise in ears; skin cold, blue, and corrugated. Treatment—Opiate, dose gr. vj., external heat, &c. Recovery on the fourth day; no fever.

DI. Magrath, æt. 50. Diarrhœa and vomiting (serous) for seven hours before admission; kidneys acting; voice feeble; respiration laboured; pulse 40, and feeble; tongue cold; skin cold and clammy; tinct. opii, ℥j., external heat, &c. Discharges ceased. Recovery; no fever.

Mary Roche, æt. 30. Diarrhœa and vomiting for two days (the former gruelly, the latter rice-water); no action of kidneys for three days; voice feeble; respiration natural; countenance sunk; skin cold; no cramps. Treatment—pil. opii. No. 2 (gr. ij.), external heat, &c. Vomiting checked; diarrhœa continued; abdomen so much distended got enema terebinth.; bowels moved so frequently (rice-water), got pil. opii, No. 4 (gr. iv.), and afterwards required enema c. tinct. opii, ℥j. Recovery on the fourth day; no fever.

Julia M'Carthy, æt. 30. Diarrhœa and vomiting for twelve hours, both gruelly at first; afterwards diarrhœa (serous), very frequent; no action of kidneys for two days; voice feeble; respiration slow; pulse very feeble; tongue natural; skin inclined to be cold; cramps in hands very severe; great noise in ears. Treatment—Opiate, dose gr. vjii., external heat, stimulants, &c. Discharges not checked for twelve hours; got enema, tinct. opii, ℥j.; improved in every way after. Recovery on the fourth day.

Bessy Lyons, æt. 30. Diarrhœa and vomiting for three days, first serous, afterwards green water; no action of kidneys for three days; voice feeble; pulse distinct but thready; skin cold; cramps. Treatment—Opiate, dose gr. vj., stimulants and external heat; bowels checked, vomiting continues; enema c. tinct. opii, ℥ij. Recovery on the fourth day.

Cor. M'Carthy, æt. 48. Diarrhœa and vomiting (serous) for two days; no action of kidneys for twenty-four hours; voice very feeble; tongue coldish; skin cold; no cramps. Treatment—Opiate, dose gr. vj.; reaction in four hours; kidneys acted on the third day. Recovery on the fourth day.

Julia M'Carthy, æt. 40. Diarrhœa and vomiting for three days (serous); no action of kidneys for forty-eight hours; voice very feeble; countenance sunk, deaf, and noise in ears; pulse very feeble; skin inclined

to be cold; no cramps. Treatment—Opiate, dose gr. iv., external heat, stimulants, &c. Discharges checked; but required enema c. tinct. opii, ℥ij.; kidneys acted on the fourth day. Recovery on the fifth day.

EIGHT CASES—PROMISCUOUS TREATMENT—AND THREE DEATHS.

DI. Shayhane, æt. 30. Diarrhœa and vomiting (purely serous); no action of kidneys for forty-eight hours; voice inaudible; respiration very slow and sighing, with pain under right breast; countenance sunk; pulse not to be felt; tongue icy cold; skin cold, clammy, and blue; cramps very severe. Treatment—Saline (Dr. Stevens'), external heat, &c. Death in twenty-four hours.

Cath. Casey, æt. 35. Diarrhœa and vomiting for about a week (waterish); no action from kidneys for two days; voice feeble; respiration slow; pulse very feeble; skin cold and clammy; cramps troublesome. R. ol. Ricini ℥j. trœ opii, gr. xxv., aq. m. pip. ℥iss., stomach being full and distended. Recovery on the second day—no fever.

Eliza Nowlan, æt. 25. Diarrhœa and vomiting for the previous night (waterish); abdomen distended; no action of kidneys for twenty-four hours; voice feeble; pulse feeble at forty; upper and lower extremities cold; cramps very bad; R. ol. Ricini, ℥j.; c. tinct. opii, gr. xxv.; foment abdomen, external heat, &c. Recovery on the third day; no fever.

Chas. Murphy, æt. 16. Diarrhœa and vomiting (profuse and serous) for two days; no action from kidneys for twenty-four hours voice feeble; pulse feeble; tongue natural; skin natural; cramps in fingers and legs. Treatment—R. mist. magnesiæ c. spt. ammon. aromat. et tinct. opii, ℥j. ad ℥viij. Required towards night ol. Ricini ℥j., c. trœ opii, gtt. xx.; vomiting relieved. Recovery on the third day.

Eliza Bateman, æt. 30. Bowels confined, and abdomen distended and hard; vomiting frequently (serous); no action of kidneys for twenty-six hours; voice very feeble; pulse 60 and feeble; skin moist and rather warm; cramps in hands and feet; R. enema terebinth.; stimulants and external heat; R. hyd. sub. gr. iij. pulv. opii gr. $\frac{1}{4}$ ft. pil. Recovered.

Jno. Leary, æt. 12. Diarrhœa and vomiting (serous) for five hours no action of kidneys for twenty-four hours; voice very feeble; pulse 60, and very feeble; tongue ice cold; skin blue, cold, and cramps. Treatment—Saline, external heat, &c. Death in sixteen hours.

Honora Shea, æt. 17. Diarrhœa and vomiting for six hours and a half (serous); no action from kidneys for could not say how long voice feeble; pulse feeble; pulse 80, and feeble; tongue natural; skin warm; abdomen full; R. ol. Ricini, ℥j., c. trœ opii, gtt. xxx.; external

heat, &c. ; required enema commune et hyd. sub. gr. ij., pulv. opii gr. $\frac{1}{4}$. Recovered ; no fever.

Jane Mercer, æt. 30. Diarrhœa and vomiting (serous) frequent before admission but worse after ; no action of kidneys for three days ; voice feeble ; respiration slow ; no pulse ; tongue cold ; skin cold and livid, and pain at epigastrium. Treatment—Saline, sinapisms, external heat, &c. Death in two hours after admission.

The following are cases treated in the Cork Fever Hospital, *during the present outbreak*, in addition to those alluded to in my observations :—

Oct. 29th, 1866. Five o'clock P.M. Cath. Reardon, æt. 35. Diarrhœa and vomiting (serous) for three days ; no action of kidneys during this time ; is nursing ; pulse distinct ; deaf. Treatment—Opiate (dose gr. vj.), external heat, &c.

Half-past nine P.M. No discharges ; pulse 40 ; no urine ; surface improved in colour and heat ; no sleep ; says "her voice comes through her ears."

Oct. 30th. Nine o'clock A.M. Pulse 60 ; no discharges ; sensation in ears gone ; kidneys acted (fourth day) ; broth drinks *ad lib.*

Six o'clock P.M. Kidneys acted again ; pulse 80 ; convalescent.

Nov. 6th. Twelve o'clock, A.M. Mary Roche, æt. 30. Diarrhœa and vomiting since about seven o'clock this morning, (blackish water) ; cramps ; surface very cold and shivering ; face bluish ; kidneys acted since admission. Treatment—Opiate (dose gr. viij.).

Five o'clock P.M. Pulse 96 ; skin warm and perspiring freely ; voice very feeble.

Eleven o'clock P.M. Pulse 96 ; good reaction ; no losses ; no urine ; quiet, but not sleeping ; very thirsty ; inclined to nausea ; to have drinks *ad lib.*

Nov. 7th. Nine o'clock A.M. Slept tolerably well ; pulse 40, feeble ; vomited once (green water) ; bowels moved once (brownish water) ; kidneys acted ; brandy two ounces ; beef-tea and drinks, *ad lib.*

Five o'clock P.M. Vomited once (greenish) ; no action of bowels ; skin warm and perspiring copiously.

Nov. 8th. Half-past nine A.M. No action of bowels ; vomited once (greenish) ; pulse 96 ; slept well ; kidneys acting ; feels quite well. Discharged to convalescent ward.

Nov. 9th. Half-past two P.M. Cath. M'Gann, æt. 40. Diarrhœa and vomiting all night ; says "she did not think she would live till morning" (serous) ; cramps in stomach ; got at home, before leaving for hospital, tinct. opii ℥ lx.

Five o'clock P.M. No discharges since she got the dose; pulse 90, and perspiring; "buzzing in ears." To have drinks *ad lib*, external heat, &c.

Nov. 10th. Nine o'clock, A.M. Slept well, no losses; kidneys acted at six o'clock this morning (first since admission); beef-tea, &c.

Five o'clock P.M. Continues to go on well. Convalescent.

Nov. 11th. Nine o'clock, P.M. Honora M'Carthy, æt. 10. Diarrhœa and vomiting for eight hours (serous); tongue cold; surface livid; pulseless; voice very feeble; no urine. Treatment—Pil. opii, No. 2 (gr. iv.), stimulants, external heat, &c.

Nov. 12th. Nine o'clock A.M. Passed a yellow watery fluid; nurse could not say what it was; vomiting (serous); bowels once moved; no pulse; mist antichol, ℥ij. (opii gr.j.) sinapison epigastrio.

Five o'clock P.M. Vomited once; no purging; pulse 100 and distinct; no urine. To have spirit. æth. nit. ℥j. in half tumbler of water, beef-tea, &c.

Nine o'clock P.M. No discharges; skin warm; pulse distinct. No urine.

Nov. 13th. Ten o'clock A.M. Pulse 100; kidneys acted just now (third day); countenance good and surface warm; drinks *ad lib*.

Five o'clock P.M. Vomited once; pulse good; chicken broth, &c.

Nov. 14th. Half-past ten A.M. Bowels moved twice (watery). Haust. tinct. opii gtt. x.

Nov. 15th. Quarter to ten A.M. Going on well; but erysipelas of face.

Nov. 16th. Convalescent.

Dec. 3rd, 1866. Half-past twelve o'clock A.M. Mary Douglas, æt. 11. Diarrhœa and vomiting (serous) for forty-eight hours; kidneys acted a little, shortly before admission; surface blue; voice good; pulse barely perceptible; extremities cold; tongue scarcely warm; eyes sunken. Treatment—Opiate, pil. opii, No. 2 (gr. iv.); external heat; sinapison epigastrio; drinks *ad lib*.

Four o'clock P.M. Vomited twice; bowels moved once (serous); pulse perceptible; surface improved. Spt. veni. gallic ℥j., spt. æth nit. ℥j., aquæ ℥iv., chicken broth, &c.

Half-past eight P.M. Pulse good; surface warm; vomited twice (serous); no purging; countenance better; drinks *ad lib*.; spt. æth. nit. in water, &c. Mist. antichol. ℥j., si necesse sit.

Dec. 4th. Half-past nine A.M. Passed a good night; vomited twice; bowels moved once (serous); kidneys acted; countenance good; surface warm and moist; pulse 96, and good strength; no inclination whatever to sleep; cont. omnia.

Four o'clock P.M. No discharges since morning; kidneys acted again; slept a little naturally.

Dec. 5th. Quarter-past nine A.M. No vomiting; bowels moved once (naturally); pulse 110, good strength; kidneys acted again; convalescent.

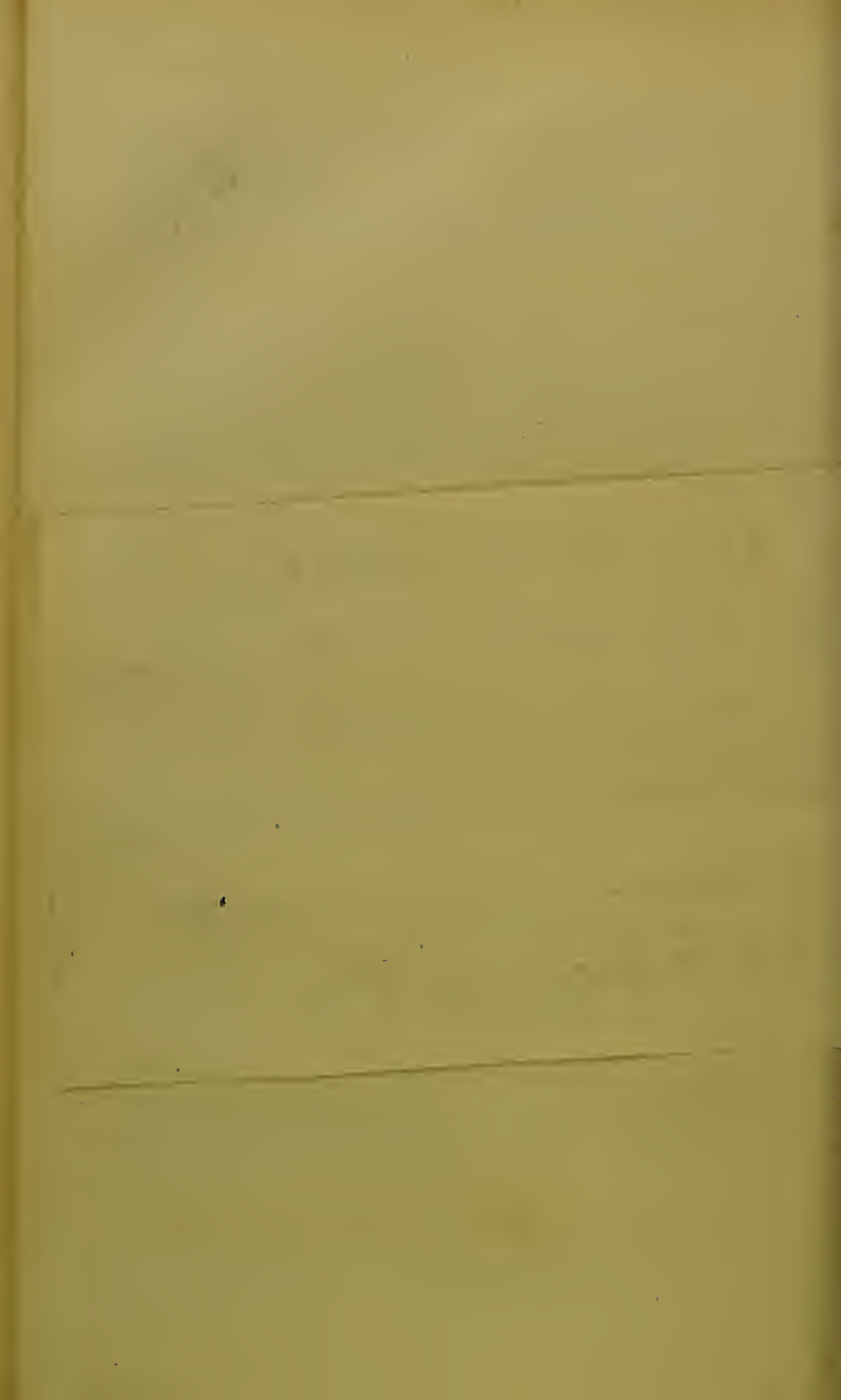
Before sending for publication I received a pamphlet, entitled, "Thoughts on the Present Theories of the Algide Stage of Cholera," by Dr. Cockle, ably written, and well worth perusal, from which I have taken the liberty of copying the following, according to Boudin, "Traité de Georap. et Statistic Medicales," p. 367, 1857, the mean mortality of the different modes of treatment is thus stated:—

Evacuant treatment	-	77·7	in 100
Stimulating „	-	54	„
Alterative „	-	36	„
Astringent „	-	20	„

In conclusion, I beg to say that in publishing the results of my experience in this disease, I have been actuated solely by three motives:— 1st. That of inspiring confidence (the best preventative against the disease) in the minds of the public with regard to its being within the reach of the profession. 2ndly. To try and reduce its treatment to a fixed and regular system based upon broad medical principles, matured and corrected by experience. And 3rdly. To caution the public against what is empirically called "infallible or specific remedies"—no physician understanding the nature of disease will pretend to such, and in cholera, particularly, time is too precious to be trifled with.

If, therefore, I succeed in the objects I have in view, I shall be amply recompensed for the loss of time and trouble a sense of public duty has obliged me to incur; as in a question involving the lives of thousands every private feeling should be merged in the one consideration, what may be best for the public good. Let it then be distinctly understood that cholera, like all other diseases (I speak from a professional experience of thirty years), formidable as it is, can be combated with a fair share of success by the resources at the disposal of medical science, if timely, properly, and actively treated.

The Senate & Academics
of The University of Glasgow
with the Author's grateful
recollections of his studentship
in that University
28th July 1839.



at 30
4
THE

SANITARY REFORM

OF

THE BRITISH ARMY.

BY

Φιλοστρατιώτης.

THIRD EDITION.

"The Army is a part of society employed, it is true, in services of a peculiar nature, which require a peculiar organization, but not on that account cut off from the general mass of the community."—COUNT RUMFORD.

"Nothing can be so extravagant as to kill good men, who have been trained at such an expense."—*Speech of the Right Hon. SIDNEY HERBERT.*

LONDON:

W. AND R. CHAMBERS, PATERNOSTER ROW.

1859.

THE HISTORY OF THE

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LONDON :
FEED AND PARDON, PRINTERS,
PATERNOSTER ROW.

TO MAJOR-GENERAL

SIR RICHARD AIREY, K.C.B.,

&c., &c., &c.,

QUARTER-MASTER GENERAL OF HER MAJESTY'S FORCES.

SIR,—There is no one who has the permanent welfare of the Soldier and the efficiency of the Service more sincerely at heart than yourself, and there is no one who can estimate the value or practicability of my suggestions for the Sanitary Improvement of the British Army better than you can : it is for these reasons that I take the liberty of dedicating the following pages to you, with the very sincere respect of

Your obedient Servant,

THE AUTHOR.

PREFACE TO THE FIRST EDITION.

I HAVE, for many years, observed with sincere regret, the deplorable ill health and consequent inefficiency of the British Army, composed of men with steel hearts and physical elasticity which no enemy could daunt—men who were ready and willing, at any moment, to lay down their lives in their country's cause.

A sort of shame comes over me while I write these lines, that I should have looked on so long and have done nothing to remedy this monster evil, leaving it to the Army Medical Department, whose especial duty it was, to rectify this inhumane mismanagement of our brave fellow-countrymen. Indeed, I had hoped from day to day, and from year to year, to see some modern HOWARD rise up to investigate the evil and suggest the remedy; but up to the present moment, my hopes have not been realized. The only indication of a tendency to this most desirable end was the appointment of a Royal Commission of Inquiry last year, which resulted in the publication of a vast amount of information, most startling and astounding to those who had not given much attention to the subject. Could it be supposed that, in this nation of Patriots and Philanthropists, our brave and matchless soldiers, pouring out their blood, not to support a despotic sovereign on the throne (for, thank God, we have a liberty-loving and truly amiable Monarch), but to maintain our rights and liberties at home and abroad, should have been treated with such thoughtless and reckless cruelty and inhumanity, and at a waste of treasure to this over-taxed nation, of several millions sterling a year; in fact, more than one-half of the cost of the army has thus been wasted!

I anxiously inquired of the military authorities, and also caused my friends to make inquiries in both Houses of Parliament, to ascertain whether anything was about to be done to remedy this lamentable condition of our Army, but no satisfactory answer could be obtained.

After painfully observing the old adage verified, that "what is everybody's business is nobody's business," I ventured to take an exception to this general rule, and on the 4th of last March I addressed a letter to His Royal Highness the Commander-in-Chief, at the Horse Guards, and on the 19th of the same month another letter to the Secretary of State for War, at the War Office; but these communications have hitherto remained "dead letters" in both departments. Several members of the Houses of Parliament, to whom I have mentioned the circumstance and the contents of these letters, advised me to have the one addressed to the Commander-in-Chief published, which I certainly should have done, but I had not kept a copy, and upon inquiry at the Horse Guards the original could not be found. I had, therefore, no alternative but to embody the contents of them, as far as I could remember, in the form of a pamphlet, which I now beg, with much deference and respect, to present to the public, in the fervent hope of affording some information and offering some useful and practical suggestions on a subject which all must admit, in the present position of this country, of the very utmost national importance.

Φιλοστρατιώτης.

WILTON HOUSE, REGENT'S PARK,
August, 1858.

PREFACE TO THE SECOND EDITION.

THE first Edition of this Pamphlet being exhausted, and the very favourable and encouraging reception given by the Public and the Press to the suggestions advanced by the Author, have induced the issue of a Second Edition. Moreover, although these suggestions have been before the military authorities for more than a year, and the *mortality in the army is still on the increase*, nothing whatever has been done to remedy this very serious national evil; forcibly recalling to mind the well-known fact, that reforms, however vital and important, seldom begin in official quarters; the unmistakable voice of public opinion, however, must, ere long, lead to real and permanent improvement.

The Author therefore hopes that as his Pamphlet is being read and extensively circulated throughout the British Empire, he may live to have the happiness of seeing his suggestions carried out to their full and practicable extent, which will give him more real pleasure and satisfaction than the highest honours could bestow.

WILTON HOUSE, REGENT'S PARK,
May, 1859.

PREFACE TO THE THIRD EDITION.

THE important facts in reference to desertions from the Army which have come to light since the Author entered upon the great object of his anxiety, stimulate his hopes of an early attention in the proper quarter to his plans of sanitary reform. What can be supposed the reason for 20,360 soldiers running away from the service within the last year, at a loss to the country of nearly two millions sterling, but a painful sense of discomfort?

It affords the writer no little pleasure to find that public sympathy so soon demands a third edition of his *brochure*. He now, with some confidence, looks for the practical co-operation of those humane and patriotic persons who have been emphatic in their approval of the measures recommended.

WILTON HOUSE, REGENT'S PARK,
July, 1869.

THE SANITARY REFORM OF THE BRITISH ARMY.

“On ne saurait croire, combien une petite santé, bien conduite, peut aller loin.” *Reveille-Parise.*

So long as Great Britain occupies her high position among the nations of the earth, so long as her colonies and dependencies are to be found in every quarter of the globe and in every clime. so long as the jealousies, the passions, and the interests of other governments make war a possibility, so long must the British Army be maintained in its efficiency.

Independently of the gratitude which, as a nation, we owe to those who have fought and bled for us, and for our homes, our firesides, and our liberties, it becomes a question of the deepest importance how to repay their devotion by increased comfort, how best to maintain and add to their efficiency, and yet, while so doing, regulate the needful expenditure economically and judiciously.

It is, however, a painful consideration, that while the nation has been most desirous to do all that could be wished, while the House of Commons can by no means be accused of parsimony, the military authorities have not always been judicious in the application of the funds placed at their disposal; nor have they benefited by experience, nor turned to account the increasing knowledge in scientific improvements within their reach. The uninterrupted peace which prevailed from 1815 to 1853, left the military administration so secure in their repose, that their organization became a system of lifeless routine, unequal to grapple with or remedy the most flagrant evils, even when pointed out to them. That they were well known, and that zealous subordinates urged remedial measures, there is ample evidence to prove; yet it is deserving of observation, that the worst and most dangerous nuisances then complained of, have not been remedied to this day.

A Royal Commission, appointed in May, 1857, has conducted an inquiry into the sanitary condition of the Army, and from a great mass of evidence, has published a report embodying many excellent recommendations and suggestions. The facts adduced on examination of witnesses, prove a state of things most startling, and exhibit a recklessness and waste of human life, and an amount of ignorance, under the circumstances, most inexcusable and unpardonable.

The Army, strange to say, in peace at home and in its usual state of health, exhibits a degree of mortality much greater than is shown by any class of civilians, including the most dangerous

and sickly employments, even that of miners, who are hourly exposed to accidents of various kinds, and to the most deadly influences.*

The greatest perils are not those of the battle-field. History, poetry, and fiction describe, in powerful and affecting language, the carnage of the field, the siege, the deadly breach, and the march, and call up before the mind's eye scenes of horror, relieved only by the halo of glory thrown around them; but the Report of the Royal Commission, in pitiless arithmetic, at once demolishes the popular delusion, and exposes, in all the nakedness of unadorned truth, the horrible fact that the mortality of the British Army is so inconsiderably swelled by losses in action, that, compared with the silent and gradual waste of life at home and in peace, "it is not worth mentioning;" that the Minié ball, the shell, the rocket, the bayonet, and the sabre, are less deadly than pestilence and disease, originated and perpetuated by inattention to the commonest rules of medical hygiene.

If ever sanitary science had a favourable field to operate upon, it exists in our army. In recruiting all are rejected (nearly one-third of those offered) who are weak, sickly, or exhibit seeds of latent disease; those accepted are further reduced in number by invaliding; those who show symptoms of breaking-up, and those left, are still further reduced by the discharge of men who have served the period for which they enlisted, and whose deaths (76 per 1,000) swell the returns of the Registrar-General, and not those of the Horse Guards.

Yet, with all these advantages, twice as many men die year by year in the ranks of the army, as in the classes of the civil population, from which they were taken, and on whom the rejected lives, to the number of 57,381 in ten years, from 1842 to 1852, have been thrown back.

Colonel Sir A. M. Tulloch, K.C.B., in his replies (6367—6369) states the civil population loses, from 20 to 25 years of age, at 9.6 per 1000, while, in the Cavalry, the mortality shown is upwards of 11 per 1,000, in the Line, 18 per 1,000, and in the Guards, upwards of 20 per 1,000, and this has been increasing of late years; till the Grenadier Guards now stand at 21.5 per 1,000! A general officer informed the author, that when he took the Guards to the Crimea they could stand no fatigue—they were, to use his own words, "perfectly rotten." On inquiry as to the cause, his reply was, "*Drunkenness, and dissipation in London.*" This only confirms the opinion given by Louis Cornaro more than three hundred years ago, "There cannot be health or long life without perfect sobriety."†

This fearful state of things has been ascribed to various causes,

* "Why," asks the Hon. Sidney Herbert, "should the profession of arms entail on those who adopt it a higher rate of mortality than almost any other profession?"—The answer to this important question will be found in the following pages.

† "*Discorsi della Vita Sobria.*" PADUA. 1558.

but reduced by the Commissioners to three distinct heads:—*Night duty; want of exercise and suitable employment; intemperance and vice, indifferent ventilation, and a crowded state of the barracks; exposure to nuisances, arising from defective sewage, regulation, and cooking.*

Careful examination proves, that although to each division are distinctly traceable serious results, they are all, more or less, deducible from three primary and monster evils:

Inproper food and cooking; bad barrack accommodation; and want of useful employment; and it is asserted that remedial alterations in these particulars will effect those most important desiderata, the health and efficiency of the various arms of the service.

NIGHT DUTY has been much exaggerated as one of the chief causes of ill health. As contrasted with the London policeman the soldier is less exposed to the inclemency of the weather, less vigilance is required of him as a fixed sentinel; nor is he exposed to conflicts with the drunken ruffianism of the metropolis, hand-to-hand fights with burglars, or the chase of thieves through lanes and alleys, whose breakneck peculiarities are better known to the pursued than to the pursuer.

The principal feature in the night duty seems to be lying down to sleep in their wet clothes; but this seems a matter so easily remedied, resolving itself into the provision of a few pegs on which to hang, and the means of drying, those garments, or even additional guard greatcoats, that it is no wonder a sad smile should pass over the features of common-sense civilians, that these petty evils should be allowed to continue, and form, in the opinion of Colonel North, "the causes of much of the illness and mortality of the Foot Guards."

Indeed, after a careful investigation of this section of the mortality causes, including the latter item, so easily admitting of remedy, the Commissioners dismiss the subject, by attaching to it very little importance:—Exposure to night duty merely stimulates into active energy the seeds of latent disease, caused by want of *proper barrack accommodation in suitable localities*, absence of *useful employment*, not merely military, and the *enervating mental and bodily effects produced by ennui*, all tending to reduce the conservative and reparative powers of the system, "*Ex viribus vivimus*," and inducing susceptibility to disease.

In regard to *intemperance*, the soldier is found to be not more drunken than the classes from which he is taken, and the evidence only proves the truism that, like other men, he would be improved by greater abstinence from intoxicating liquor. *Ennui* and the situation of his barracks, and still more the absence of *useful employment for his leisure hours*, have much to do with the prevalence of this habit, so demoralising mentally, and so destructive to the constitution, by producing disease of the digestive organs and nervous system, and causing *more than nine-tenths of the cases of lunacy.*

Vice, veiled by the terms, "dissipation of other kinds," in plain language, sexual debauchery, it is admitted, prevails among soldiers to a greater extent than among young men of the same age, from the mass of the population. Here, again, the same causes appear as the root of the evil, and are rightly ascribed, by the Report, to the soldier's fixed residence *in towns and cities*, with their greater facilities for debauchery. The number of lives lost and of men whose health is yearly sacrificed to this cause, swell the returns of mortality and "unfitness for service," amounting to 206 men per 1,000 in Cavalry, and 250 per 1,000 in Infantry, permanently affected with venereal disease of all kinds (9570). It is asserted that more than 5 per cent. of our army are in hospital, or off duty, from *this cause alone*: and of these, *not one half are ever again fit for duty*. Soldiers never apply to the surgeon till disease has made very serious inroads on their health, supervening venereal rheumatism, in the majority of cases, rendering the soldier morbidly susceptible of cold, which any additional clothing he may put on cannot prevent. It is much to be desired that the surgeons should fraternise a little more with the men, which I think they might do, without in any way lessening the dignity of their position, and thus inspire these poor fellows with a more friendly confidence in their medical attendant, so that proper attention might be given to the disease in its early stages, where a speedy and radical cure might then be hoped for.

A medical friend of mine had once the temporary charge of some troops. On delivering his report, he was asked how many deaths he had. "None." How many cases of illness?—"Not one per cent." How was this? Had he omitted to put down any? "No," said he, "but I used the best means I could to *prevent* illness, and made myself a sort of father of the family, and the soldiers made known their little ailments to me as soon as they occurred, and most of them I could stop at once; they were very submissive and grateful, and we became reciprocally attached to each other, and disease found no encouragement amongst us."

There have been competitive examinations for superior qualifications of surgeons, might there not be competitive medical returns for superior health of the soldiers? surely the one is quite as much deserving of a trial as the other.

On the subject of CLOTHING:—A little attention and common sense on the part of the Clothing Department, to the climate, duty, and quality of the materials supplied, would soon rectify the errors in this direction. The more healthy the constitution, the more independent of the defence of clothing:—the more the conservative powers of the human frame are reduced, the more necessity for the defence of clothing.

No one can look at a soldier in heavy marching order without mingled feelings of commiseration and disgust at the manner in which the poor fellow is loaded, with a square box on his

back, to which he is strapped in a way that compresses the pectoral, scapular, and dorsal muscles (the chief instruments used in moving the arms), and strangles the blood-vessels and nerves in the axilla, rendering his arms almost useless, and his breathing difficult, and tending no doubt to induce consumption.* Anything so monstrous, so barbarous and cruel as this "regulation knapsack," on a long march, it is hardly possible to imagine. The application of a little mechanical skill might materially lessen, if not wholly remedy, this very serious evil. Again, it may be asked, what can the Medical Department be about, whose duty it certainly is, or ought to be, to see that the soldier's physical action is free from impediment or hindrance of every kind? Next in importance, are the boots:—Every one must have noticed the crippled condition of the soldiers after a march of only a few miles, arising from the ill-adapted boots they are compelled to wear. These boots are constructed of the most rigid and unyielding materials, under the mistaken notion that rigidity implies strength and durability. This is a great error: a pliant boot, made of sound materials, will last much longer. Every mechanical appliance to the human frame ought to follow, and not impede, the healthy mechanical action of the part to which it is applied. If this fact were more generally known and acted upon, much pain, suffering, and disease would be avoided, and the action of the human frame would be more free and more graceful.

The question of DIET demands and has received greater attention:—As to quantity and quality of what is supplied, the diet of the soldier appears far superior to that of the agricultural labourer, but there the superiority ends. Variety in kind and preparation is absolutely necessary to insure health (5262). The soldier is provided with neither change in kind nor in cookery; he is often condemned to live on boiled beef for twenty-one years. The constant repetition of the meal of soddened meat becomes so disgusting that the soldier, in fact, throws his food to the dogs; out of $\frac{3}{4}$ lb. allowed, not more than half is consumed, and, of course, the soldier falls off in strength. His disgust prevents him from consuming and digesting the quantity required to keep up "the athletic constitution," or that which is capable of great and continuous muscular efforts, as in prize-running and other similar feats. Soldiers are, in every respect, physical-force men, and will be the more efficient the nearer they are brought to the athletic constitution; but as the demand for protracted manual exertion occurs only at intervals, the highly nutritive athletic dietary is not necessary. It must, however, consist of $\frac{3}{4}$ lb. of carboniferous and $\frac{1}{4}$ lb. of nitrogenous principles, in whatever way, or in whatever materials supplied, and of not less than 23 ounces of real nutriment. Vegetables ought, therefore, to form a considerable proportion of the soldier's rations. In the

* Vide Dr. Lebeau, Physician-in-Chief to the Military Hospitals of Belgium.—*Hospital Report.*

present arrangements, these are difficult to be had, and are not, therefore, used in quantities at all equal to their necessity and importance.

The object of this pamphlet being to point out the radical defects under which the military medical hygiene labours, and to exhibit the remedial alternative, the supply of so essential an element in the dietary of the soldier, will be adverted to in the means proposed for the relief of the monotony of the soldier's life.

In considering, further, the causes of mortality in all branches of the army, and the means of remedy, BARRACK ACCOMMODATION becomes one of the most important.

Barracks were originally erected in cities and large towns to serve three purposes:—To avoid billeting, to encourage recruiting, and for the repression of popular disturbances. The last object is superseded by the efficiency of the police and the rapid conveyance by railway.

The concurrent testimony of all who have been examined, officers and others, shows that they are a disgrace to the age in which we live, to that department of the service to which they more particularly belong, and exhibit an apathy to their continuance in their present state, an ignorance of architectural arrangement, and an utter defiance of the commonest rules of sanitary science, for which there can be no excuse offered, nor by the nation accepted.

In reply to questions as to the cause of the excessive mortality, Sir Richard Airey, (3158, 3166); Colonel Jebb, (5227, 5248); Colonel Tulloch, (6421); G. R. Dartnell, (8675, 8679); Sir J. M'Neill, (9833, 9841); R. Rawlinson, (3314, 3317); The Hon. Colonel Lindsay, (5829, 5833) and others, condemn the construction, ventilation, sanitary and other arrangements in no measured terms.

The fact is, that the barracks generally are most repulsively inhumane, defective in all sanitary arrangements, and even in those matters that make up the "decencies of life;" and the result is, that the soldier lives by day and sleeps "by night in a fetid and unwholesome atmosphere, the habitual breathing of which, though producing, for the most part, no direct perceptible effects, probably lays the seeds of that pulmonary disease which is so fatal to the British Army."

Of the state of this atmosphere, abundant evidence of a most disgusting nature is to be found in the statements of the witnesses before the Barrack Committee.

Such being the case, it is evident that among the causes of disease in the Army, the defects of all kinds in the construction of Barracks are the greatest, the most constant, and most fatal in their results; and yet large sums have been voted by the representatives of the people, a great portion of which has been productive of but little benefit to the soldier or the nation. In any deficiency in the comfort of the troops, the House of Com-

mons is not to blame. During the last thirty-four years, no less than £7,500,000 have been expended, of which, in 1854, 5, 6, & 7, £3,134,633 were for the construction of huts and for the enlargement and repairs of barracks at home; in other words, patching up old barracks on the old system, and building others which are very questionable in their construction, devoid of comfort, ventilation, and proper sanitary arrangements.

A further sum of upwards of a million has been voted within the last two years for the same purpose. Is it wise to spend this amount in the same way? Certainly not! Like all that has gone before it, the apparently unfathomable slough of the present barrack accommodation will swallow it up, leaving nothing of value to show for its expenditure.

It is impossible to improve the barracks in *large cities* so as to fit them for the purposes intended, unless at an expenditure which would be as appalling as it would be absurd.

What, then, is the remedy?

The barracks, in many parts of the kingdom, are *placed in the centre of populous districts*, and in most unhealthy and inappropriate situations. These barracks ought to be sold, and as the sites are often of great value, the returns would be considerable. New buildings ought to be erected on the most approved principles of sanitary science in open spaces in the country, affording room for exercise, ventilation, the erection of workshops, and, if possible, also, the cultivation of agricultural and horticultural productions.

The minimum cubic space allowed to each soldier is 450 feet, but, on the average, there is a deficiency of one-third, and often more than one-half, while it is considered absolutely necessary that, at least, 600 feet be afforded. How is this to be obtained? Is it by extending the present barrack accommodation? A moment's consideration will serve to show that the sum now voted, large as it is, would go but a very short way, as the value of the additional ground required in cities and large towns, (very likely covered with buildings) must be taken into account. But, indeed, the space allotted must be relative to the situation:—300 cubic feet in barracks placed in open country sites, will afford more oxygen—more health-sustaining, blood-purifying air—than 3,000 cubic feet of space in crowded, ill-ventilated localities, where every inhalation is loaded with miasmatic and carbonaceous impurities.

Pulmonary diseases form by far the largest proportion of the deaths and discharges from the army. Great surprise is expressed that such should be the case. A little consideration will, however, change that surprise into greater astonishment that they should be so few under the present mismanagement.

“Consumption and scrofula,” says my friend the learned and distinguished Dr. M'Cormac, “in all essentials are one. Tubercle, in its varied protean guises, is but the result of a deterioration of the blood, of the retention of excretions, carbonaceous and

other impurities of the blood, where they ought not to remain. In consequence of the imperfect performance of the respiratory functions, these impurities accumulate. The time at length comes round when they must be got rid of, if not during and through the act of respiration, per force or otherwise. The result is, their deposition as an inorganic matter in the form of tubercle, in the lungs and other organs, these, saving the diffusion of tuberculous blood being perchance, in other respects, sound, a dead matter is deposited in the tissues."

These all-important facts being determined, can it be looked upon as surprising that the soldier, fed on an undue proportion of carboniferous food, sleeping by night and passing his days in ill-ventilated barracks, breathing an atmosphere loaded with carbonaceous impurities, in localities defective in sewage and sanitary precautions, and without general muscular exertion out of doors, or even in-door *employment* combined with *mental exercise*, should acquire that consumptive tendency which it is the apparent endeavour of the regulations of the service to communicate, by pouring into the system, in every way, carbonaceous impurities—the causes of the disease.

Even presuming the extension of barrack accommodation by additional buildings effected in their present situations, confined, ill-ventilated, and ill-drained, by no talent of the engineering department could the buildings be made uniform, convenient, or comfortable.

The erection of new barracks here recommended would afford increased convenience and comfort, much better ventilation through more open space, better sanitary arrangements, provided the medical topography be attended to in the choice of a site, and improve the soldier both physically and morally.

Intemperance and sexual debauchery would be very materially decreased, the opportunities being less to receive and communicate disease; and this opinion is corroborated by the Report of the Commissioners:—"There is, doubtless, a greater amount of dissipation of other kinds among soldiers than among young men of the same class in civil life. Their residence *in towns offers great facilities for sexual debauchery*, and the diseases which are thereby generated, the existence of which the soldier, from one cause or another, frequently conceals, thereby greatly adding to the intensity of the malady and the difficulty of the cure, as well as the necessary severity of the treatment, no doubt, have a most injurious effect on his constitution."

Pulmonary diseases, rheumatism, and other complaints traceable to causes originating in venereal disease, sewage, and improper sanitary arrangements, form the great majority of those cases which prove mortal, or are discharged from the army, and are clearly deducible from the improper barrack accommodation in the inclusive sense, and from the residence of the soldier in towns and cities.

A radical change is, therefore, absolutely necessary. Not one

shilling of the large sum of upwards of a million lately granted ought to be expended, save in temporary and trifling repairs. The plans sent into the War Office during the sitting of the Barrack Committee, in 1855, ought to be examined by a committee of practical men, including military officers, engineer officers, medical officers, and gentlemen well acquainted with sanitary arrangements, and buildings erected in accordance with their recommendations, and at a distance from towns, on sites, the medical topography of which is unexceptionable. (9545.)

The only plausible objection which may be offered against their being placed at a distance from towns, is the want of markets for the soldiers. This is more specious than real. *The more an army is assimilated in time of peace to its conditions in war, and made dependent upon the commissariat for its supplies, the better for the soldier,* and the less we shall be harrowed by the description of such scenes as occurred in the Crimea.

It is folly, nay, it is worse, it is unjust, to accuse the authorities at the Horse Guards of inattention to the lives and comforts of the men. It is notorious that His Royal Highness the present Commander-in-Chief, as well as those able and intelligent men under him, exert themselves in every way, and take a deep interest in the well-being and permanent advantage of the British soldier; but they are, to a great extent, powerless, restrained by routine and adherence to departmental regulations. They may, and often *have* represented a state of facts which generally are admitted, and have been met by the answer:—"All true and very desirable, but Government has no money." Rather than face complaints made in the House of Commons against further outlay, there has been a pandering to the cry of economy, and a risking of the results—a course of procedure false in policy, false in economy, unjust and inhuman. The Army Medical Department and the War Office are alone to blame.

The lowest estimate of the cost of each soldier in recruiting and training is £100. "Nothing," says that amiable and distinguished statesman and philanthropist, the Right Hon. Sidney Herbert, "can be so extravagant as to kill good men, who have been trained at such an expense." It is, besides, a mistake, and a libel on the good sense and humanity of the House of Commons, to say that it will grudge any needful sum required for the comfort and efficiency of the Army, and to save the fearful mortality now so prevalent. They have already shown that nothing is further from their intentions and wishes. The most earnest economist will oppose no grant which he is satisfied will be properly expended for the permanent benefit of the Army? What has been grudged and most justly opposed, is the waste of money thrown away in peddling, unsatisfactory, and useless works, having no beneficial or satisfactory evidence to show for the expenditure. In this opposition they are and will be supported by the country, and by every thinking man who has not a chance of profiting by such reckless and unprofitable outlay. But England

is too large-hearted and too generous to grudge any sum which will truly benefit her brave soldiers.

It is absurd to imagine that improvements upon a large scale can be carried out, save at considerable expense. The subject of barrack accommodation is far too important, and too interesting to the nation, to be any longer allowed to remain without alteration, more especially when the remedy is one of *economy* and *humanity*. Every expenditure which will economise the soldier's health, and conduce to his comfort, is national thrift.

So much, then, for physical improvements; but Col. Jebb goes further, and says he believes that "making a soldier comfortable in his quarters is indispensable, as the basis of all training by which the moral standard of the army may be raised," an opinion which finds a responsive echo in the experience of every man who has devoted any attention to the moral improvement of his fellow-men.

The cause of mortality, as indicated by the Commissioners, which remains unnoticed, is *the want of exercise and suitable employment*.—More than half the deaths in the Army are caused by consumption, which Mr. Neison, the eminent actuary, ascribes to *want of healthy exercise*;—*Vide* Neison's paper read before the British Association at Leeds.

That the proper employment and exercise of mind and body, are both necessary and conducive to health, no one will dispute. The absence of these requisites, all must admit to be full of danger to the well-being of the human constitution. That exercise formed an important part of the scheme of the Almighty in the construction of the human frame, is evidenced by the fact that we possess 248 bones, and nearly as many joints, and these are moved by 436 muscles. If, therefore, these moving powers be not exercised, they become feeble, diseased, and decay: an important law of nature for the conservation of health is in this way infringed.

"Every muscle of the human frame must live," says a distinguished author, "and it is one of the natural functions that the muscle is to be called into action; and if it is not so, it will assume the form of disease immediately."

To the absence of healthful exercise of mind and body, and the situation and condition of the barracks, may be traced most mischievous results, which press on and destroy the soldier in peace and at home, impair the efficiency of the Army, and form so heavy an item (twelve millions) in the taxation of the country. The historical records of campaigns inform us, that even in war there are periods when armies suffer greatly from monotony, and that the spirits and health of troops improve, on the near prospect of action.

In peace and at home the duty is that of a garrison, and the routine becomes injuriously irksome. Col. the Hon. James Lindsay, than whom few have devoted more attention to the wants and the improvement of the soldier, bears the following

testimony : — “ Perhaps no living individual suffers more than he from *ennui*. He has *no employment* save his drill and his duties. These are of a most monotonous and uninteresting description, so much so that you cannot increase them. In consequence of the nature of his position, the soldier has necessarily a *great deal of idle time on his hands*, and of course if he is idle, it naturally leads him into mischief; at least he has an opportunity of getting into mischief. If you were to give the men more military occupation, more military duty, it would disgust them, and you could not do that; you cannot employ their time in a greater amount of duty than at present, except at Aldershot, where they are throwing up works; if you do that, you will disgust them, and the reason of that is this, that all the soldier has to do is under restraint. It is not like a working man, or an artizan: a working man digs and his mind is his own, and an artizan is interested very likely in the work in which he is engaged; but a man in the position of a soldier must give you all his attention, and there is a great physical demand on him in consequence of the weight he carries. All these things make me think that it would be impossible to ask him to do more duty or drill than he is now called on to perform. It ought to be the *duty of government to give him pursuits inside the barracks*, instead of tempting him outside.”

Without undervaluing in the slightest degree the cultivation of intellectual pursuits, the supply of libraries, reading-rooms, and newspapers to the soldier, it must not be forgotten that the men are drawn from a class not much inclined to value very highly the acquisition of knowledge beyond mere reading, writing, and arithmetic; and that a taste for learning and habits of study are seldom, if ever, acquired at an age similar to that when recruits begin their education in military day schools. Mental cultivation is inimical to physical development and muscular strength. “Intellectual cultivation,” says Dr. James Johnson, “sows the seeds of physical deterioration; and the evils inflicted on the flesh fail not to grow up, and ultimately retaliate with interest on the spirit. The besetting sin of the present generation is that of reading and thinking. A vigorous circulation is necessary to a vigorous contractility, that is, health and strength; and that vigorous contractility is incompatible with a high degree of susceptibility. One of the first effects of civilization is to substitute the labour of the brain for the labour of the hands and feet. Bodily imbecility and enfeebled health are invariably consequent upon a sedentary life; all go to prove that man was destined to live a life of physical activity.”

The duties of the soldier are made up of drill, parade, and sentry duty, consuming but a portion of his time, and leaving several hours on his hands unemployed. The effects of this life are more marked in some corps than in others. There is more monotony of occupation and more leisure time in the Infantry of the Line and in the Guards than in the Cavalry:

consequently the foot-soldier spends but a small portion of his time "in exercise or out of doors." The Cavalry soldier has more varied duties to perform. Stable-work, grooming his horse, riding as well as walking, bring different sets of muscles into action, and his spending a greater number of hours in the open air, all tend to the greater benefit of his health.

Agricultural labourers are frequently held forth as instances of longevity, notwithstanding their inferior food, lodging, and personal cleanliness; and they have been contrasted with the soldier, who, at first sight, appears more favourably situated. In certain respects the soldier enjoys numerous advantages over the civilian and agricultural labourer; but they are counter-balanced by evils to which he is condemned by regulation. The real solution of the problem is the *out-door healthful employment* in which the agricultural labourer is engaged.

Somewhat alive to the necessity for muscular exercise, the authorities have provided and encouraged athletic games and amusements, and have even dreamed of importing some of those practised by our lively neighbours. This is a thoughtless error. "*Vive la bagatelle*," is not characteristic of a Briton. Manly sports are very well in their way, and as an occasional recreation; but they are not felt to be useful or satisfying to the mind of a practical and industrious Englishman. Many of the men will not even indulge in the amusements provided for them, purely because they are not profitable. A Briton is at heart a utilitarian animal, and this important fact must never be lost sight of in our efforts for his progressive improvement. Give him work and proper encouragement, and he will do it. In this, his manly character stands forth, worthy of all praise. He must be improved in the direction of his natural tendency; and that being industrial occupation, it becomes the imperative duty of the authorities to find useful and healthful employment for those leisure hours which, free from military duty, are now misspent in idleness, drunkenness, and dissipation. For his labour, the soldier *should be paid* according to the kind and quantity of work done, in addition to his usual pay. This proposal of industrial occupation for the soldier has received the approbation of many distinguished members of both Houses of Parliament, and of military officers of high distinction, who have devoted much time and anxious attention to the wants and requirements of the soldier; amongst others may be mentioned the names of Sir Richard Airey, the Honourable Colonel Lindsay, Colonel Jebb, and Colonel Tulloch.*

Physiology, experience, common sense, and the anxious

* "Besides the schools of instruction, as they were called, schools of industry were established in the regiments, where the soldiers and their children were taught various kinds of work, and from whence they were supplied with raw materials to work for their own emolument; as nothing is so certainly fatal to morals as *habitual idleness*. Every possible means was adopted

craving of the mind for occupation and the frame for exercise, prove incontestibly the soundness of the views herein propounded.

The question arises—how, and in what way, are they to be employed? No doubt, many modes of employment will suggest themselves to the intelligent reader. The first object ought to be, the *removal of barracks from cities and large towns to open and healthy situations in the country*. Waste lands well situated, the medical topography being good, would afford employment for a portion of the men inclined to agricultural and horticultural pursuits. The erection of workshops connected with the barracks, wherein the men may pursue the trades requisite for the manufacture of their clothing of every kind, accoutrements and small arms of every sort, would afford modes of employment most desirable, and indeed necessary, not merely on the ground of the immense saving in the economy of health, and in the production of the different articles required by the soldier, but would make him less helpless and more independent when he has occasion to go upon foreign service. The land around the barracks so cultivated would produce fresh vegetables at a low rate, so necessary and indispensable for the use of the men, besides improving and preserving health, by the cultivation of the land, as well as affording the means of preserving the health of others.

No doubt there may be difficulties to encounter in the general introduction of these changes, but these are mere questions of time and arrangement—no doubt, many prejudices to overcome, and existing interests to be abolished; but these sink into insignificance when compared with the immense advantages which would accrue to the soldier and to the nation.

The labour or employment afforded to the soldier, if only paid at the rate of three halfpence an hour, for *four* hours in the day—and it is asserted that he has, on the average, double that time on his hands unemployed—would amount to *three shillings a week*. This sum placed in the regimental savings' bank, during the twenty-one years of his service, would produce, with interest accumulations, upwards of £250, to be paid to him on his leaving the Army. If employed on piece-work, the amount might, in all probability, be double or treble that sum. But, allowing for all drawbacks in the way of time otherwise taken up by duty, changing of quarters, occasional illness, &c., still the amount acquired during the twenty-one years of service would be very large in money for his situation in life, and the benefit still greater in health, morals, and industrious habits.

Morally and religiously, the advantages would be very great—that could be devised to introduce a spirit of industry amongst the troops. Every encouragement was given to the soldiers to employ their leisure time, when they were off duty, in working for their own emolument. The effect of this plan was much greater and more important than I could have expected."—COUNT RUMFORD.

indeed. Dissipation and crime would be decreased: men steeped in either do not readily listen to religious instruction, or the truths which affect their eternal welfare; the relations of the chaplain and the men would become nearer and more satisfactory—a sober and industrious man is predisposed to be both moral and religious.

The Army is at present, in a great degree, the receptacle for the idle, lazy, dissolute, and often also of some more questionable characters.

Independently of the great physical benefit conferred on these men as soldiers by healthful mental and bodily employment, they would “chiefly gain in having acquired habits of industry instead of habits of idleness;” habits which, once formed in military life for a period of twenty-one years, they would never be likely to relinquish, but carry with them into private life, when discharged from the Army at the average age of forty years.

Of the value of this agency as an inoculating medium of the class from which he sprang, when again returned to it, and of the estimate in which the retired, industrious soldier would be held, as contrasted with the discharged of the present day, no accurate calculation can be formed.

Possessed of a sum of money acquired by his industrial, non-military avocations, he would be looked up to; parents would have no hesitation to bestow the hands of their daughters on such men. So guaranteed in the possession of capital, character, a trade, and with these industrious habits, there are few small tradesmen, at a similar time of life, however careful, who arrive at an equally favourable position in society.

Instead of the Army being looked upon, as at present, by parents, as an idle, dissipated life, with very poor eventual provision for those who survive the period of their service, it would be considered as a preparatory training-field for sobriety, good conduct, industrious habits, the acquisition of a trade, and, united to a retiring pension for service, the means of acquiring whereon to retire into, or embark in the business of, civil life. Instead, therefore, of the lies, the trickery, and the despicable artifices resorted to for recruiting, and the difficulty of retaining the men after enlistment, until conveyed to the dépôt, the “unadorned eloquence” of the recruiting serjeant would become useless: each retired soldier would become the centre of a small recruiting district; his position, his habits of sobriety and industry, would be the best and most eloquent recommendation to his young friends to follow his example, and “go and do likewise.” But the value of this retired soldier, in a national point of view, does not end here. His patriotic zeal, his respect for the service from which he had derived so much kindness and benefit, would make him a *National Guardsman* in cases of emergency or invasion.

The space of this pamphlet does not admit of more than a

mere enumeration of the advantages derivable from the proposed improvements in the sanitary management of the Army, which are immense, indeed incalculable. It may, however, be mentioned, that soldiers preserved from disease in the Army, the children of such retired men would not inherit and perpetuate those diseases which curse and enfeeble the constitution of others differently procreated.

The nature of the employment has been already adverted to, and may shortly be recapitulated. It should consist of the manufacture of all articles required by the troops, embracing every trade, and also the cultivation of waste lands, and the growth of vegetables for the men, and forage for the horses. I need not repeat how much this would conduce towards health, strength, and improved position in every way. In fact, the English Soldier should be a sort of practical Robinson Crusoe, and not the weak and helpless individual he was found to be at the Crimea, where our army must have perished, had not the energy and talent of Dr. Sutherland and his colleagues come to their rescue.

"I have," says Count Rumford, "endeavoured, in all my operations, to unite the interest of the soldier with the interest of civil society, and to render the military force, even in time of peace, subservient to the *public good*. To facilitate and promote these important objects, to establish a respectable standing army *which should do the least possible harm to the population*, morals, manufactures, and agriculture of the country, it was necessary to make soldiers citizens, and citizens soldiers. The effect of this plan was much greater and more important than I could have expected. The soldiers, from being the most indolent of mortals, and from having very little knowledge of gardening, became industrious and skilful cultivators, and grew so fond of vegetables, particularly of potatoes, that these useful and wholesome productions began to constitute a very essential part of their daily food. These improvements began also to spread amongst the farmers and peasants through the whole country. There was hardly a soldier that went on furlough, that did not carry with him a few potatoes for planting, and a little collection of garden seeds; and I have already had the satisfaction to see little gardens, here and there, making their appearance in different parts of the country."*

With such a system of self-supply, we should hear no more of the *want* or the *waste of stores*, decay of materials, and sales of valuable effects at a sacrifice of 90 per cent., bought back by contractors only to be re-supplied to the authorities at their original cost! and less of the enormous fortunes acquired by army contractors, and of unfit and inferior articles supplied by manufacturers at a yearly cost of hundreds of thousands of pounds to the nation, and undue pressure on the industrious tax-paying community.

* Life of Count Rumford.

Such radical and important changes cannot be carried out suddenly, but a beginning may be made, and that without a week's delay. The necessity is a crying one. To continue the present state of affairs, is wasteful, unjust, oppressive, and inhuman. The remedies proposed are proved to be practicable, and are guaranteed effective by military, medical, and civil experience. The field for a commencement is not wanting, but is most opportunely and happily found in the camp at Aldershott. The situation of this encampment in its medical topography is excellent; the subsoil, with proper drainage, unexceptionable; but *without proper and scientific drainage*, the ground may in a few years, with a population of twenty or thirty thousand persons in a small space, become saturated with pestilential matter. It affords ample space for the erection of workshops. The heath land (upwards of eight thousand acres) offers room for the utilization of manure, and the employment of many thousands of men in healthful agricultural and horticultural pursuits, men who are now wasting their energies in *ennui*, idleness, and dissipation. In my visits to Aldershott I have several times mentioned the subject to the men, and they readily and heartily responded, saying they would be happy to employ their leisure time in such occupations, if even allowed nothing for their labour. Let the opportunity, then, be afforded them, and I can fancy the gladness, the hearty and loud cheers that would answer the call to industrial occupation. The example of the industrious would soon shame the idle, the lazy, and the dissipated, into similar efforts, tending to their own moral regeneration, the great good of their country, and the economy of the national outlay. By such means Aldershott, now considered by the whole Army as the "*soldier's penal settlement*," might be made his *sanitorium* and his *pleasure ground*, affording him useful, profitable, and agreeable occupation for his leisure hours, and an abundant supply of wholesome vegetable productions, so necessary to his health, and which he cannot now obtain at any reasonable cost.

One more suggestion demands immediate attention by the military authorities, and is implied in the reforms herein recommended: *the establishment of a well-organised and efficient sanitary staff in the camp of Aldershott*. Nature has made the situation healthy, but the neglect of sanitary science will soon render it pestiferous. The permanent barracks are not well placed, and will require active and vigilant sanitary supervision. It is well observed by a distinguished writer on sanitary science, that "what is wanted in the army, is intelligent sanitary advice, and the means of giving it immediate and practical effect."

These reforms, if carried into effect, will, to some extent, take the sting from the remark, that "in amassing riches, and playing the philanthropist to all the world, England neglects those to whom she is indebted for wealth and power."























Letter

ON

THE CLIMATE OF MADEIRA.

J. M. ^{Esq} Bloxham. 1854.

Funchal, Madeira,

1 June, 1854.

DEAR DR. LUND,

I have undertaken to put down upon paper some of my ideas on the subject of Medico-meteorology, with reference particularly to the climate of Madeira. You, who know my taste for scientific pursuits, may probably imagine that I am much better qualified than I am, in fact, to furnish you with some valuable hints. In the first place, my knowledge of medicine and of diseases is as limited as is well possible, since it is confined to that which I have unavoidably acquired during some years of experience—not as a physician, but as a patient. Of this, however, you cannot be entirely ignorant. Secondly, wherever I have given my attention to medico-meteorological writings, I have been struck with what appears to me to be the extremely unscientific manner in which the indications of meteorological instruments are dealt with, and this has created in me a distaste for the pursuit of a science which I can scarcely hope to advance. It appears to me, that the value and use of such indications are so imperfectly understood, that they have yet to be ascertained, by careful and laborious observations made in localities the effects of the climate of which, in originating, aggravating, and alleviating, particular diseases are known by independent means; but, that the generality of persons who have *written* on the subject, seem to think that a few hygrometric and thermometric data are sufficient to enable them to pronounce that a climate of long-established reputation, whether good or bad, is in fact the

reverse of what was previously supposed. Moreover, it seems to be a matter of utter indifference, that such data are often obtained from extremely defective instruments, observed by persons whose habits and pursuits have not qualified them for the task ; and who can scarcely be supposed to have had sufficient leisure to enable them to devote to it the degree of laborious and minute attention that it requires. But so it is, that persons who choose arrogantly to adopt and advocate any peculiar views upon a subject which is sufficiently obscure to admit of being so dealt with, and who are not seeking philosophically to investigate the truth, require only data upon which to found their arguments ; and, whether those data are true or false, is to them a matter of too little importance to be worth the trouble of investigation.

What the climate of Madeira really is, seems to be an interesting and much-disputed question. I fancy that the opinions of the medical men of the present day, in England, are founded in great measure, either directly or indirectly, upon Dr. Mason's *Treatise on the Climate and Meteorology of Madeira* ; and that certain advocates of peculiar views have, by one-sided quotations, contributed, in no small degree, to make that book convey impressions very different from what the author intended ;—to say nothing of the high estimation in which Dr. Mason's observations seem to be regarded by those persons, and the very little credit which is given to them here, not by medical men alone, but by all persons whom I have heard speak of them, and whose knowledge of meteorology in general, or of Dr. Mason's observations in particular, render their opinions of any value.

The editor of Dr. Mason's book, page 199, suggests, that if visitors to Madeira would employ a part of their leisure in recording the state of the weather, the difference of opinion, now (1850) existing, as to the advantages or disadvantages of its climate, in certain cases of disease, would disappear. I cannot help thinking such records would contain a large mass of contradictory evidence, calculated to be useful to advocates of any views whatever, except the truth. The suggestion, however, is useful as an admission of the insufficiency of Dr. Mason's observations to settle those differences of opinion. It also furnishes a remarkable instance of implicit and exclusive faith in meteorology.

logical data, as means of determining the *effects* of a climate upon diseases; for the editor apparently does not think, that medical reports would be necessary or even useful adjuncts to such records. The same gentleman (page 200) also suggests that the medical practitioners resident in the island can hardly allow the subject to be longer neglected, in the present advanced state of science, without subjecting themselves to the reproach of indifference, relative to the charge sometimes urged against them, of withholding the truth, under a dread that the far-famed climate of the island will not bear the test of close and accurate examination. This reads to me something like charging a man with not looking at his barometer on a fine day under a dread that its index will point to "foul weather." (And, by way of parenthesis, let me ask, why should the medical practitioners be charged with withholding that which is patent to every one who chooses to investigate the subject, and for which investigation the editor seems to think every invalid visitor is sufficiently well qualified?) But, in truth, we have long since learnt that those amusing little indications which instrument makers still continue to put into the mouth of the barometer, are not to be trusted; and I should have thought that, by this time, the hygrometer need not be similarly falsified. The barometer is a very valuable instrument; but, as has been justly observed, it has been brought into disrepute by the absurdity of engraving the words "fair, foul, wet, dry," etc., at different parts of the scale; and really one would suppose that some of our medical men are prepared to write the words "consumption, fever, cholera," etc., at different points of the hygrometric scale, and to prescribe climates for their patients accordingly. And here I must quote from the *Athenæum* for 1853, page 248, the following extract from a recent publication, by T. H. Burgess, M.D., which the *Athenæum* gives as the summing up of the author's experience; for this article has led to the present discussion:—

"It results, from the preceding statements, that much misconception prevails with respect to the efficacy of *foreign* climates in cases of pulmonary consumption; and, however agreeable to the senses warm air, sunny skies, and luxuriant vegetation may seem, they afford no proof of salubrity, nor of the beneficial

effects of any climate. Madeira, with all its sanitary fame, is no exception to this rule, as the meteorological observations of Drs. Heineken, Gourlay, and Mason incontestably establish. Malta, etc., etc.”—In the name of common sense, how can meteorological observations incontestably establish any such fact? Do the numerical readings of the barometer, hygrometer, etc., or the points of the compass towards which the wind-vane happens to point, afford proof of the salubrity, or of the *beneficial effects* of climate any better than what is agreeable to the senses? Taking this as a specimen, I can easily believe that it does result from the “preceding statements,” referred to in the above quotation, that much misconception prevails *somewhere*; for it seems that, because we are told that the hygrometers of the above-named observers gave certain numerical results, we must, without further inquiry, either respecting other facts, or as to the amount of credit to be given to those statements, or whether the observations were general or confined to particular localities, take it as a fact *incontestably* proved, that the climate of Madeira has not, and never had, that effect upon invalids which both the public and the medical profession attribute to it. And, strange to say, in order to arrive at this *incontestable* conclusion, we must read those cabalistic numbers according to the interpretation of the author in question; for no one else, not even the three meteorological observers themselves, who, be it remembered, were members of the medical profession, were able to discover that they conveyed any such meaning, as the following quotations fully show.

Dr. Gourlay* writes as follows, except that I have underlined some words which, in the original, are not printed in italics.

At page 31, “The salubrity of the climate in this island, so highly extolled, is greatly attributed to the *uniformity of its temperature*. A regular succession of land and sea-breezes, cool and purify its atmosphere during the whole year, and, especially, during the hottest months. Hence, a drop of dew seldom falls,

* Observations on the Natural History, Climate, and Diseases, of Madeira, during a period of Eighteen Years. By William Gourlay, M.D., Fellow of the Royal College of Physicians, Edinburgh; and Physician to the British Factory at Madeira. London: 1811.

except in the higher parts of the island; and any deleterious effluvia, which may arise from the surface of the earth, or from other sources, are dissipated as soon as they are produced."

At page 32, "During the day, the whole range of the thermometer will seldom, at any season, exceed two, or at most four degrees, and frequently, for several days together, the same degree of heat is indicated.

At page 33, "Where such uniformity of temperature exists, combined with purity of atmosphere, and where such a pleasing variety marks the climate, one would conceive that the inconveniences of seasons would be unknown; and that neither the excessive heats of summer would molest, nor the colds of winter pinch, the frame; but it is found that this pleasing picture is not *entirely* realised; and though it may be truly said that, in general, spring and autumn compose the whole year, yet it is not to be concealed that, during the months of *July, August, and September*, which are the hottest months, the heat becomes excessive and intolerable; and that, on one or two occasions, the winter has been distinguished by a severe storm. Still, however, the winter may be said to be known only, perhaps, by a gale of wind, which may drive the vessels in the roads from their anchorage, or by a torrent of rain, which produces a rapid flow of the rivers down the ravines." It should be observed, that July, August, and September are not included in the Madeira season, that very few English invalids remain in the island during those months, and that those few usually remove to higher and cooler residences during the heat of the summer.

At page 90, "Madeira, from its *uniformity of temperature* and purity of atmosphere, has long been, and still continues to be, the favourite retreat of consumptive patients from the northern parts of Europe. Here, the unhappy sufferers under this formidable disease *cheat the winter of their own climate*, and gain that cessation of suffering which such a situation is fitted to produce."

At page 92, after describing the class of patients who had been ordered to Madeira, "Before such patients repair to this *last haven* of health, their malady is unfortunately, in too many cases, in its last stage; when neither change of climate, nor any remedy whatever, can be of service. From what cause this back-

wardness to an earlier trial of a southern climate proceeds, is not for me to determine; but it would be well if the physicians of such patients were to recommend a change of temperature in the first stage of the malady, where, etc."

I have been unable to procure a copy of Dr. Heineken's meteorological observations, but the following extracts from a letter of his, dated Nov. 1826 (the same year as that in which his meteorological observations were published) and printed in the appendix to Mr. Lyall's book,* will answer my present purpose.

At page 334, "Dr. Price estimated the expectation of a child at birth, in London, to be nineteen years of life, and Dr. T. Heberden, in Madeira, thirty-nine years. Without, however, stopping to inquire into the accuracy of these estimates, or the probable causes for their very great disproportion, allowing them to be correct; it is certainly true, that Madeira is remarkably healthy: from most of the diseases peculiar to warm climates, it is exempt; and many of those which in more northern latitudes, from the frequency of their occurrence, and epidemic or endemic characters, become a scourge, are here either altogether unknown or but slightly felt."

At page 337, "I shall take for granted, that my medical brethren in England will only advise those who are likely to benefit by climate to quit their native shores; and, with this proviso, I do not hesitate to say that Madeira holds out advantages that are not to be met with combined in any other quarter of the globe."

At page 339, "The temperature of Madeira is more equable (contrasting day with night, and summer with winter) than that of any other place. Our rains are violent, almost tropical; but they are also periodical and circumscribed, and never lingering and teasing. We are entirely free from the piercing keen winds which are met with, more or less, all over the continent of Europe, and enjoy, throughout almost the whole summer, although more partially than between the tropics, "the trades" and land and sea-breezes which there prevail."

Dr. Mason says (page 37): "The observations made at Sta. Luzia apply to that locality alone, and cannot in any way be made to suit the island generally; nor will they give precise

* *Rambles in Madeira and in Portugal.* London: 1827.

information relative to the lower part of Funchal, near the sea, especially as regards the progress of humidity during the day, although, in point of temperature, they may be very near the truth....I may also remark that almost every locality offers something peculiar alike with regard to temperature, humidity, or the local winds which prevail; and that until a series of observations shall have been made in different localities, the full merits of the climate, as regards the suitability to different diseases, or even stages of the same disease, will never be fully ascertained," etc. On the same page he states that invalids generally reside above the town, in the same line in which his observations were made. This may have been the case some twenty years ago, when Dr. Mason wrote, but it is very different now.

At page 133, Dr. Mason says: "From this statement (a long one, which it is unnecessary to quote), we can account for the fact that patients who visit Madeira are so differently affected by the precisely same conditions of the atmosphere; some experiencing relief, and others only an aggravation of their complaint. Accordingly the present work will be regarded, not as an attempt to prejudice that island as a resort for invalids, but as an effort to point out the danger of an *indiscriminate* reliance upon the sanitary effects of its climate. Such a reliance is injurious. The not unfrequently frustrated hopes of anxious friends, suggest advantages, real or imaginary, from a resort to other localities; whereas the atmospheric phenomena of Madeira being ascertained, and the requirements of the patients found to correspond therewith, comparative uniformity of success would establish its reputation; and the failure of cases to which its climate is not adapted, would not be attended with the effects of damaging its character as a residence for those who, by a change to such a locality, might reasonably calculate upon the realization of their most sanguine expectations."

Again, at page 152: "Those who, on their arrival, find the climate disagrees with them, had better immediately remove to a drier climate; while those with whom it materially disagrees, as indicated by the symptoms which I have described, may rest assured that they will derive permanent benefit from remaining, that their hopes will not be blighted, but that returning health

and strength will result from leaving for a season their own less hospitable climate."

At page 162, writing on the climate of London: "In its effects on the animal economy our summer season will approach to the Madeira climate, being slightly modified by temperature and hygrometric condition."

Notwithstanding these opinions of Drs. Heineken, Gourlay, and Mason, it is now clearly ascertained, as we are informed by Dr. Burgess, that their own *meteorological observations* incontestably prove that the sanitary fame of Madeira is a pure delusion. The hieroglyphics which those gentlemen merely placed upon record have now been decyphered, and their meaning admits of no further dispute. It is ascertained, moreover, that Dr. Mason was mistaken in supposing that those which he found at Sta. Luzia Cottage were applicable to that locality alone.

The words "luxuriant vegetation", in the extract which I have quoted from the *Athenæum*, call for some remarks. The books which have been written on Madeira contain many statements on this point, which, without being more than commonly exaggerated, are calculated to entirely mislead a stranger as to the *general* character of the island. So far as my knowledge extends, there is scarcely a spot to be found where there is moisture sufficient to support luxuriant vegetation, without the assistance of artificial irrigation. Sir H. Davy, as quoted by Dr. Mason (p. 41), alludes to the great quantity of basaltic rock uncovered by vegetation. It is true that artificial irrigation is carried to a great extent; nevertheless the more general character of the surface of the country is dryness, barrenness, and absence of luxuriant vegetation. Even grass for horses and cows is brought down *daily* from the mountains; and in the summer, even on the mountains, the grass becomes so dry that a spark of fire will endanger the whole district. The whole island is of volcanic origin; its surface is either precipitous, or very much inclined, and the soil is of a porous nature. With the exception of a volcanic crater, and one or two other spots at distant parts of the island, none of which have I ever seen, I believe there are no hollows or basins capable of retaining water; and those to which I allude as the exceptions, do so only for limited periods. I have never seen a lake or even a pond.

There are some yam-grounds which, I should suppose, must be injurious to the health of those persons who live on or close to them; but they are quite unworthy of further notice in this place.

The lestes of Madeira are sometimes made great bugbears. We may judge of Dr. Mason's opinion of their *importance* to invalids, by the passage which I have already quoted, in which he advises those persons with whom the leste materially *dis*agrees, to *remain* in the island. With respect to his advice that those who, *on their arrival*, find the leste agrees with them, had better *immediately* remove to a drier climate, I have to remark, that the season here is considered to begin on the 1st Oct., and to end on the 31st May. Now I arrived in the middle of September 1850, and it was not until some time in the following June that I had an opportunity of judging of the effects of a leste; and my experience of lestes during the three succeeding seasons has not been much greater. The precise and strong symptoms which Dr. Mason describes as distinguishing these winds, may occur sometimes, for anything that I know to the contrary; but most assuredly not always. It is not unfrequently a matter for doubt and difference of opinion, whether or not there is a leste; and the strong symptoms which Dr. Mason describes, I have never witnessed in the course of four *seasons* and three summers.

The principal point in dispute respecting the climate of Madeira, is, whether it is dry or damp. Persons who judge of it by the test of their own feelings, use such expressions as the following: "The air is soft and delicious, and strikes with a peculiar charm the stranger, whom, perhaps, a few days have transferred from the gloom and chill of an English winter." "The dry and balmy air which produces this never-ending spring, makes the step buoyant, and raises the hopes of the sufferer, who a few days before left the choking fogs, the rains and chilly damps, of the Thames and the Medway." Dr. Mason, however (page 33), says, "It would be a difficult task to convince many of the residents that the climate is at all damp, although the fact admits of being proved in the most satisfactory and philosophical manner." Let us admit, for the present, that Dr. Mason has proved in the most satisfactory and philosophical

manner that the air of Madeira is very damp, that is to say, damp in philosophical language; and let us suppose also, for the present, that in philosophical language a damp air is one that contains a more than average quantity of moisture, the measure being the number of grains of water in a cubic foot of air; for this, as I understand, is the manner of estimating the dampness of a climate to which Dr. Mason alludes. Are we to infer from these admissions, that persons who describe the climate, manifestly with reference to its effects upon the sensations, are labouring under a pure delusion? Is it not obvious, on the contrary, that the sensation of dampness depends upon something besides the number of grains of water in a cubic foot of air, and that Dr. Mason's satisfactory manner of estimating dampness fails to detect that not unimportant something, whatever it may be? Philosophers, in their arguments with the unlearned, frequently contrive to beg the question. So, in the present case, Dr. Mason (or his predecessors, it is immaterial which) first appropriates the words dampness and dryness, and, by implication, defines their meanings to be in accordance with his method of estimating those qualities. Then, secondly, he has no difficulty in proving the ignorance and obstinacy of all who persist in calling the air damp or dry, according to the original but less definite application of those words. And, thirdly, he absolutely ignores those differences in the state of the air which are not indicated by his method of estimating it. Nevertheless, I do not believe it has ever been ascertained, nor does Dr. Mason assert, that dampness, as measured by his method, furnishes a truer criterion than our own sensations do, of the *suitability* of a climate for animal life. His method has the advantage of being a definite and philosophical measure of something; whilst the other is vague and uncertain. But the philosopher has no right to rest satisfied with his method. There are other qualities of air, of which we have sufficient evidence; and it is his business to discover, define, and estimate them. It is most unphilosophical simply to ignore those other qualities, because they cannot, as yet, be defined or measured by any known instrument. Such distinctions as those to which I have just alluded would enter into theoretical speculations, but would be of little practical importance, at present, if medico-meteorologists would

always be content to deal fairly with the question. Dr. Mason tells us that his method of estimating dampness gives materially different, nay contradictory results, when compared with non-instrumental estimates; but he does not fall into the error of appropriating to a damp climate, as estimated by his method, all the ill effects which either experience or prejudice has attributed to dampness, as tested by the other means. For this latter step we are indebted to other persons, who raise the cry of vapour, moisture, luxuriant vegetation, dampness, etc., etc., in the ears of persons who are affected by a species of hydrophobia; then appeal to Dr. Mason and other authorities, for certain facts which answer the intended purpose, and carefully suppress the opinions of those same authorities, as to the value of those facts and the inferences to be drawn from them.

Dr. Mason tells us (page 18) that the mean temperatures of the air of London and of Madeira are about 50° and 68° respectively, and consequently that, when saturated, the air of the latter contains twice as much moisture as the air of the former. Another inference, which we are equally at liberty to draw, is that the air of London, when absolutely saturated with moisture, is as dry as the air of Madeira when in a state of only half-saturation. Now, people of ordinary understanding would certainly call the former excessively damp, and the latter excessively dry. Nevertheless, we must bow to our medical philosophers (not Dr. Mason, but others of more recent date), and not merely admit that, in such a case, there would be equal quantities of water in the air of both climates, but also that, so far as regards animal life in general, and all diseases in particular, which are usually considered to be much affected by dryness or dampness, the two climates are to be considered as on a par—that the air of London, when in a state of absolute saturation, is on a par with the air of Madeira in a state of half saturation! Surely, this must be quite conclusive against the climate of Madeira, especially when we remember that a state of half-saturation corresponds to a depression of the dew-point equal to about 18° , whilst Dr. Mason allows us (see p. 26 and table XXXII) a mean annual depression of 7° at most. There is, however, another way of estimating the dryness or dampness of air, which is equally well known to the ordinary meteorologist; and I would

submit to your judgment whether it ought to be absolutely discarded in medico-meteorology. At moderate elevations, the air, whether in England or in Madeira, seldom is absolutely saturated with moisture. Consequently, if we understand the comparison between the air of those two places when in that state, to mean nothing more than the words literally express, the fact is of no great value. Perhaps we are intended to understand that, as a general rule, the air of Madeira would be twice as damp as the air of London. But, under the point of saturation, if the air of Madeira contains just twice as much moisture as the air of London, the former may still be considered, in another sense, twice as dry as the latter; that is to say, it is capable of absorbing twice as much *additional* moisture. Consequently, there are two methods of comparing climates: one of which might enable us to pronounce that climate A is twice as damp as climate B; whilst the other enables us to state just the reverse; and either would admit of being proved in a most satisfactory and philosophical manner. Such of our medical men as may wish to prove that the climate of Madeira is a mere delusion, and to advise their patients indiscriminately to remain in England, may adopt the former method; but they may perhaps find it convenient to be provided with the other, in case the question should be, a choice of residence in England or in the polar regions, where, undoubtedly, the air contains a very small quantity of moisture; since, if we take the average temperature of an extreme northern climate as high as 10° F., it will contain only one-fourth part as much moisture as the air of London, both being saturated.

The observation which I have quoted at the beginning of the preceding paragraph, is a casual remark of Dr. Mason, to which he attaches no more importance than it deserves: and, consequently, I should not have thought that it required to be noticed, if I had not seen that it is misused by the manner in which it is quoted, misquoted, and re-quoted elsewhere.

I shall now proceed to make some observations respecting the accuracy of Dr. Mason's meteorological observations, and their value as general results. I have learnt, from what I consider sufficiently good authority, that, during his residence in the island, Dr. Mason was several times absent from Santa

Luzia cottage, for many days if not weeks at a time. Indeed, he writes of having been at Santa Cruz. How his meteorological register was kept during those intervals, or by what process of calculation the blanks were filled up, we are not informed. We are only told, that he never for a moment deputed the task to any other person.

At page 1, he tells us that the height of Santa Luzia cottage, the place at which he made his observations, is 350 feet above the level of the sea, and at pages 82 and 83 it is stated at 300 feet. Having taken some pains to measure the height of my own house, which is only a few yards distant from Dr. Mason's house, I can state that the latter is less than 250 feet above the level of the sea, and not 350, or even 300 feet.

At page 2, he says the register-thermometer for external temperature *in the shade*, received the sun's rays *obliquely* from 2 to 5 P. M. At least, I so understand the sentence. However, he probably meant, that the stone pillar against which the thermometer was placed was so exposed; but either is very important: and, exactly what he means by the *oblique* rays of the sun, in this latitude from 2 to 5 P. M., and of course all the year round, I am at a loss to understand.

At page 3, he tells us that the direction of the wind was determined by a vane, placed upon a high staff. It is true that the staff, which still remains, is nearly 8 yards long; but it stands so much below the level of my house, and other buildings in its immediate vicinity, that I can only regard it as a useless toy. Even if the buildings were removed, the vane could be expected to indicate only the direction of the current of air in that part of the valley, and would be quite useless for general purposes. I confess I am at a loss to point out any systematic method of registering the direction of the wind, in this mountainous country, which would be at all satisfactory; but I consider Dr. Mason's register of his own wind-vane as mere waste paper. One might as well register the flickerings of a candle in a room.

A more important question is, the value of Dr. Mason's hygrometrical observations. Santa Luzia cottage is a very small house. The room in which the hygrometer was placed opens towards the garden, which is much confined by high walls, especially near to the house. The room is supported upon stone pillars, with

an open area under it and for a little space in front of it; the garden being two or three feet above the level of the area. Close to, and nearly on a level with the room in question, is an open water-tank, the vent of which leads into the area. The leakage and waste water would naturally keep the area constantly damp, and, when I saw it, such was the case. An open water-course, called here a lavada, runs across the garden at the distance of a few feet from the windows of the room in question, and quite close to the area, in fact upon the low wall which forms the limit to the area on the garden side; and this lavada, being two or three feet higher than the area, would tend to increase its dampness, both by surface-evaporation and by filtration. The garden, I am told, was in a state of luxuriant vegetation, and grew, amongst other things, bananas. When I visited the room, I immediately perceived a damp, mouldy smell, which I presume arose from the area and tank, under and in front of it. The following is what Dr. Mason himself says with respect to the tank (see page 40). After speaking of the tanks kept to irrigate gardens, "I feel fully justified in attributing to those sources the effect of poisoning the air; as I suffered severely in my own person all the symptoms generally referred to the effects of marsh effluvia—such as extreme lassitude, pains in the head and limbs, intolerance of light, mental depression and anxiety, dry, parched, brown tongue, etc.—all which disappeared in three days, without the aid of medicine, upon removing to Santa Cruz, a few miles from Funchal. On my return to Santa Luzia, the same symptoms re-appeared after a residence of a few days, and continued, unabated, till this source of annoyance was partly removed, when some amelioration of the symptoms took place. I have not the least doubt that they would have disappeared completely, could the stagnant water have been entirely got rid of; but, although my landlord had lived some years in England, I had much trouble to convince him that water could be at all offensive, after being kept two months in a tank."

From the latter part of this quotation it seems probable that, during a portion of Dr. Mason's residence, the tank was in a worse state than it is at present; and this is somewhat confirmed by the description which the late Mr. Wilkinson gave me of it;

since his description is barely justified by the present state of the tank. Mr. Wilkinson also told me that he frequently remonstrated with Dr. Mason, on the absurdity of placing his hygrometer in such a situation as the one he had chosen. On this point, however, Dr. Mason says (page 3), "The hygrometer was situated in a room to the west, between two windows constantly open from 6 A. M. to 6 P. M., and very free from currents of air, as they acted like folding doors. This room was, consequently, free from local humidity arising from the evaporation of water from the ground, etc." Can anything be more absurd than to suppose open windows, acting like folding doors, would exclude local humidity? What can be the value of hygrometrical observations with reference to the general climate of Madeira, which were dependent upon such means of excluding peculiar local influences?

At page 6, Dr. Mason says, "There is a striking coincidence in the results afforded by Dr. Heineken's observations [made eight years before those of Dr. Mason] and my own, although the instruments by means of which they were obtained are so widely different; proving that when such observations are based on facts, instruments, however varied in their constructions, must offer, upon comparison, results which accord with one another; because nature is uniform in her operations; whence the same causes invariably produce the same effects." At page 203, the editor presents us with a comparison of Dr. Mason's and Mr. McEwen's observations made with similar instruments in the same months of different years, the latter giving upon an average somewhat more than double the degree of dryness stated by the former, and accompanied by the following remarks: "The difference of locality, or the circumstance of his (Dr. Mason's) observations having been made in the house, with open windows, and mine (Mr. McEwen's) out of doors, does not sufficiently explain the discrepancy, which I think fully proves—what Dr. Mason suspected—that the different years vary much more than is generally admitted." How very consistent are these two proofs! It may be as well to remark also that the results given by Dr. Mason, with respect to the mean annual dryness on the dew-point hygrometer (to use his own expression), at nearly the same hours of the day, are as follows: Dr. Mason's,

7°.42 — Dr. Heineken's, 7°.42 — a very striking coincidence certainly, since there is not a difference of even one-hundredth part or a degree. However, since Dr. Mason's observations were not made with the dew-point hygrometer, his result, when referred to the dew-point, depends upon the factor of reduction which he employed. Now Dr. Mason employed a constant factor, without regard to the temperature of the air; but if we employ Mr. Glaisher's factor (taking the temperature of the air between 65° and 70°, since it is stated in Table XXVII at 68°. 12) the results will stand thus—Dr. Mason's, 5°. 41—Dr. Heineken's, 7°.42—the latter being 37 per cent. greater than the former. I have already pointed out, in the comparison of Dr. Mason's and Mr. McEwen's results, obtained with similar instruments, that the last is 100 per cent. greater than the first. Whether the facts on which these several results are based are erroneous, or whether nature has deviated from the ordinary uniformity of her operations, are questions which I shall leave for future investigation.

At page 31, Dr. Mason says, "In order to prove the dampness of the climate, I may instance the impossibility of keeping iron, in any form, from being rapidly oxydized. The different powders, such as opium, squills, etc., soon lose their pulverulent form, and become firmly united into a solid mass; various neutral salts rapidly deliquesce; gloves, shoes, etc., soon become covered with various species of cryptogamous plants; silks become spotted and unfit for use; pianofortes frequently require retuning; and the screws of various other instruments, as violins, guitars, etc., became so tight as to be almost immoveable. In fact, it would be impossible for vegetation to flourish, were not the atmosphere almost saturated with moisture; as frequently, during the fine season, there is scarcely a shower of rain for three, four, and sometimes even six months in succession." To take the last statement first, I do not believe that vegetation does or could flourish, during such seasons as Dr. Mason describes, without artificial irrigation. It is somewhat remarkable, moreover, that Dr. Mason should speak of such long periods of drought, since, in his table VI, there is not one month in which rain did not fall on two days and two nights at least; and, on those occasions, it rained during the whole of the two days, and only two hours

short of the whole of the two nights. The other facts I can easily imagine Dr. Mason really observed in his own house, since I have heard of similar occurrences in other houses: but I can make a counter-statement. I have now just completed my fourth season of residence in a house which is the next above Santa Luzia cottage, and only a few yards from it. Gloves, boots, and silk handkerchiefs, kept in my own room during the whole of those periods, have never shown the slightest symptoms of becoming mouldy or spotted. I have a great number of iron and steel tools, which have remained constantly in another room, ever since my arrival here nearly four years ago. Though I do find that the tendency of iron to rust is greater than I have observed in inland places in England, I much question whether it is at all greater than at many sea-side places in England. Some of the polished steel tools, which have been kept nearly four years in the same room, without being either used or cared for, remain to this day without even a minute spot of rust, so far as I have observed from casual inspection. With respect to the statements that the screws of violins, etc., become tight, I should attribute such a result, not to the general dampness of the climate, which I think could scarcely have that effect, but to the removal of the instrument from a dryer to a damper situation. The screws being properly tight when dry, might become almost immoveable when swelled by moisture. Consequently, it is easy to believe that the screws of violins, guitars, etc., always become fixed when taken into Santa Luzia cottage. Though I have not been in the habit of keeping a meteorological register, I have frequently observed the hygrometric state of the air—not with great care nor with very good instruments. The results, however, when compared with Dr. Mason's, are far from proving that “nature is always uniform in her operations:”

Admitting that Dr. Mason's meteorological observations are entitled to more credit than I give to them; yet, in a paragraph which I have already quoted, we have his own statement that his results cannot in any way be made to apply to the island generally, and that, until a series of observations shall have been made in other localities, the merits of its climate can never be fully known.

I cannot take leave of Dr. Mason without alluding to one

other circumstance.—A paragraph in the book to which I have already alluded has been pointed out to me by a lady. It mentions Dr. Mason's fate as "a telling comment on the blind credulity which prevails respecting the virtues of *foreign* climates in pulmonary consumption:" and then follows a quotation from the editor's preface to Dr. Mason's book, which, I confess, does not appear to me to furnish any such comment. But it is a melancholy and pathetic tale, and, since it seems to have touched the too sensitive feelings of the gentleman in question, it would be illiberal to find fault with his logic. Nevertheless, he can scarcely complain, if I merely contribute a little more pathos, by quoting the very first paragraph of the same preface. It is as follows, except that I have underlined some passages which, in the original, are not printed in italics.—"Apart from the value of Dr. Mason's work as affording a just estimate of a climate," (which, by the bye, both the author and the editor afterwards tell us it does not do) "the resort of a particular and large class of invalids; his labours acquire an interest from the fact of their having been prosecuted in a state of extremely infirm health, *regardless of the influence which they must have had in aggravating the symptoms, and lessening the chance of recovery.* He may truly be said to have *sacrificed his life to professional zeal.* Contending with an extensive derangement of the pulmonary functions, he resolutely *cast aside all solicitude* for his own health; and, without intermission or pause, completed a series of difficult and fatiguing observations, with the noble view of rendering a benefit to society. The *exposure and privations* which he would have imperatively prohibited a patient from encountering, he fearlessly and enthusiastically contended with in his own person; undeterred by the most *trying fluctuations of temperature,* the prostration attendant upon a constant strain of mind, and the watching which broke in upon that ordinary rest which even the robust cannot forego without some degree of suffering. To none would he, for a moment, depute the task which he had undertaken; and, when all around him were enjoying repose or courting it, this *martyr*, as he may be called, to *meteorological investigation* passed the night *with his instruments and journal,* noting down the minutest changes which the atmosphere underwent, from the first sinking of the sun to

the first indication of its rising." Alas ! what a melancholy instance is this of blind credulity respecting the virtues of foreign climates !

I do not wish the preceding remarks to be understood as indicating any opinions of my own, as to the sanitary effects of *foreign* climates in general, or of the climate of Madeira in particular, with reference to English invalids. It is a question upon which I feel totally incompetent to form an independent opinion of any value. My object has been to expose the insufficiency of the grounds upon which other persons have arrived at conclusions which they have not hesitated to pronounce with so much self-confidence. I have not asserted that their conclusions are false ; but I dispute the accuracy of the data, and the validity of the reasoning by means of which they profess to have arrived at those conclusions. Neither do I wish to assert, as a fact really ascertained, that Dr. Mason's cottage was so damp or so ill-chosen a place for hygrometrical experiments as some of my observations might seem to imply. It is sufficient for my purpose, if I have shown what I believe to be the fact, namely, that so much suspicion attaches itself to the value of his hygrometrical statistics, that no reliance ought to be placed upon them. Nevertheless, it may be as well that I should now state what my opinions are with respect to Madeira, if only for the purpose of preventing a false inference being drawn from my silence on that point. In a few words then, my opinion is, that the climate is extremely beneficial in many cases. That the cases which are likely to derive benefit from the climate can, in general, be discriminated by the resident medical men ; but that, owing partly to the effects of the climate being less well understood by medical men resident in England, many invalids are sent here who had much better have remained at home. . More-over, there are two questions which ought to be carefully distinguished : the one, whether the climate is calculated to act beneficially on any particular disease ; and the other, whether it is advisable to send a particular individual who is afflicted with that disease to this climate. For example, the same author will tell us, on one page, what a melancholy sight it is to see poor deluded individuals seeking for health abroad, alone, and away from their families and the comforts of a home ; and, on

another page, of the folly of individuals going to Italy in search of health, and spending their time in cathedrals, picture-galleries, theatres, ball-rooms, etc. : two somewhat contradictory representations, if each is to be understood as applicable to the class invalid in general ; both of which are, however, put forward as valid arguments against the abstract climate theory ; though the latter, so far as it is worth anything, furnishes an argument in its favour : for the same persons neither would nor could have spent their time so agreeably in England. That such excesses should induce greater evils than the climate can compensate for, is but too probable ; but such a result furnishes no argument against the *climate theory*. The abuses of climate, which are very frequent and well known to resident medical men, are too often overlooked or not fully appreciated by medical men in England, when they have to consider the advisability of sending a patient abroad ; and also, when they attempt to form opinions respecting foreign climates, by means of the results to those whom they have sent abroad, and who, of course, are almost certain to conceal or misrepresent every imprudence of which they have been guilty. For myself, I have reason to think that I owe my life to the sanitary effects of the climate of Madeira. I do not, of course, forget the benefits which I have also derived from medical advice, and the strictness with which I have always acted upon it.

I have expressed an opinion that the science of medico-meteorology is very imperfectly understood. I am not qualified, nor can I afford sufficient space to enlarge much upon this point ; but I shall, nevertheless, hazard a few observations, even at the risk of exciting the wrath of some pseudo-scientific members of your profession, who make many words, not in the spirit of scientific inquiry, but in that of arrogant dictation, upon a subject which, it is evident, they do not understand—which, in fact, is not understood by any one—and by them not sufficiently to enable them to see their own ignorance.

I fully admit, as a general rule, that the warmer the climate the greater is the quantity of aqueous vapour contained in a cubic foot of air. Is there any reason for doubting that this is a beneficial provision of nature ? In any given climate of limited extent, a particular spot which is more than ordinarily damp, as

measured by the proportion of aqueous vapour to common air, is usually found to be unhealthy. But if we attempt to compare places situate in very different latitudes by the same rule, it leads to results which are so preposterous that the method of comparison is obviously inapplicable. I do not believe the quantity of pure aqueous vapour in the air is a matter of nearly so much importance as it is generally supposed to be; but, that many injurious effects upon the human system, which are attributed to dampness, depend, if not entirely, at least in great measure, upon other elements. It may be that *moisture* causes those other elements to be developed, or that *aqueous vapour* acts as their vehicle. It may be that the same causes often produce both dampness and those other elements. On either of those suppositions dampness might be mistaken for the cause of the effects observed, especially when we have no precise means of detecting any other cause to which they can be attributed; and I cannot help thinking that such mistakes are frequently made. Can it be contended that the air of a close, dark, damp cellar is no more injurious to human health than equally damp air rendered so by fresh steam from a tea-kettle, or other similar means? We know that over marshes miasmata sometimes exist to such an extent as to depopulate large districts, whilst a sea-air, more than equally damp, is considered conducive to health; yet no one doubts that the miasmata are caused by the moisture of the marshes. Though the existence of miasmata in marshy localities is fully recognized, their presence cannot be detected by any meteorological instrument, or even by chemical analysis of the air; and consequently it is easy to believe that we cannot form correct estimates of the salubrity of air by any instrumental or even chemical investigation of its qualities. I feel a strong conviction that many effects which are attributed to aqueous vapour alone, are, in fact, produced by other constituents, which frequently, but by no means necessarily, accompany it; and that whether aqueous vapour is or is not accompanied by those constituents, depends upon the source from whence it is derived; perhaps somewhat in the same manner as water acquires different qualities by filtration through different mineral strata; and that it is as great a mistake to attribute those effects to the aqueous vapour, as it would be to attribute the peculiar effects

of different mineral waters to the one constituent water. A slight excess of aqueous vapour, if it happens to be derived from a poisoned source, may be a matter of great importance; and within a limited district, a slight excess of aqueous vapour at a particular spot, often does arise from such a source; and, in other cases, it is an indication of want of free circulation of the air, and consequently of an undue accumulation of noxious vapours. But no such inferences can be drawn from comparisons of the quantities of aqueous vapours in the air of places at great distances from each other; in which case the difference in the quantities of aqueous vapour is likely to be dependent upon more extensive and essentially different causes.

I do not mean to assert that aqueous vapour is an unimportant element in medico-meteorology; but, until the many other elements which enter into the question can be more certainly detected, and their effects eliminated, I think that hygrometry will be of very limited practical use. It must not be forgotten that there are many recognized meteors, such as electricity, ozone, and various gases, which can be estimated, though with more or less difficulty, the effects of which upon the human system may be considerable, and that as yet very little is known about them. What Dr. Prout says in the appendix to the *second edition* of the eighth number of the *Bridgewater Treatises*, appears to me to deserve the attention of medico-meteorologists. I refer to his suggestions respecting the opposite effects of pure aqueous vapour, and of aqueous vapour in union with oxygen or deutoxide of hydrogen; the affinity being apparently so slight, that this combination occurs only when the oxygen in the atmosphere exceeds the chemical equivalent, 1 of oxygen to 4 of hydrogen; and that the results of every *common* analysis and examination of air are the same nearly as if such a state of combination did not exist. With respect to temperature, I have no remark to make, except that we must not lose sight of the fact, to which you will strongly testify, namely, that it is not merely the general or average state of the air that determines the salubrity of a climate, but that the amount and suddenness of the changes to which it is liable are most important points, especially for invalids. Of course we ought also to take into account the question, how far particular changes, or injurious influences, necessarily

affect an invalid, and how far he may be protected from them by reasonable care. In which view of the matter, habitual heavy dews for an hour about sun-set, or even during the whole night, might be regarded as of no moment; whilst a liability to sudden changes of temperature, or a hot sun and cold winds prevailing during the day, would be extremely injurious. The equability of the climate of Madeira is, perhaps, one of its chief virtues. It would, however, be a mere delusion to suppose that it is not liable to changes. There can, I imagine, be no place on the face of the globe which is not liable to changes, some more and some less than others. It would be equally a delusion to suppose that the climate of Madeira is a specific cure for consumption; and writers on climates might, perhaps, spare themselves the trouble of proving that it is not so. No sensible person, who understands what those words mean, can for a moment entertain such an idea. I must return to my starting-point, and restate my opinion that, except in some extreme cases, the sanitary properties of climates can as yet be determined only by their sanitary effects. For data of this description we are dependent, in great measure, upon the opinions of medical practitioners, and none can be so good as those of the medical men resident at the place in question. Dr. Burgess may say, or, by quoting the editor of Dr. Mason's book, may imply, that the resident medical men being prejudiced and interested, their opinions are not to be trusted; but, in so saying, he libels the profession in general rather than the individuals in particular against whom such observations are levelled. If we are to cast aside all medical statements and opinions which are open to the same objections, what have we left to trust to, and whom shall we believe? We are not, however, bound to accept either their assertions of facts, or their opinions, absolutely in the dark. There are various means of sifting evidence and ascertaining its value, and we are furnished with a great deal on the subject of climates which will not stand the test.

When I began this letter I had no intention of making frequent reference to Dr. Burgess's book, which I had not read. By degrees I have been drawn into an examination of the Madeira portion of the first chapter, comprised in ten pages; and I must present you with some of the fruits of my investigation.

At page 11, Dr. Burgess quotes from Mr. White a statement respecting the equability of this climate, and then tells us that Mr. White *reluctantly* admits that, although so very equable, the climate is not altogether free from changes, which constitute there, as elsewhere, the exciting causes of pulmonary affections. Mr. White's words are, "pulmonary and inflammatory affections", immediately followed by these words, which Dr. Burgess also omits: "These, however, as may be supposed, are comparatively rare among the better classes, and occur chiefly among the hard-working poor, who are more exposed to this cause and to sudden chills of the surface while perspiring profusely." Why did Dr. Burgess omit this sentence? A little further on Dr. Burgess says: "Yet in this most perfect climate, the same writer informs us that the different eddies or currents caused by the vicinity of the mountains, render either a vane or anemometer of little use." A wind-vane, or anemometer, in order to be of use, must be exposed to the wind; the valley of Funchal is sheltered from the wind on the north, east, and west sides, and consequently these instruments are of little use. It is futile to employ instruments for the purpose of registering the direction or force of mere eddies and currents of air, which are to be found in every sheltered spot which is not too confined; and the valley of Funchal has the advantage of being very extensive.

At page 12, Dr. Burgess writes: "It is also stated" (by Mr. White, I presume,) "that the position of his (Dr. Mason's) instruments was not well chosen"; and coolly continues, "However, a summary of Dr. Mason's observations will enable the reader to form his own opinions as to their merits, and the reliance to be placed on them." I should have supposed that a reader who wished to form an opinion as to the reliance to be placed on the accuracy of meteorological observations, would find it necessary to examine them in detail, and not by means of a summary. Moreover, Dr. Burgess does not give us a summary of Dr. Mason's observations, but merely a few *extracts* from the editor's preface, and from the *text* of Dr. Mason's work; and with what degree of fairness those extracts are made, I am about to show.

On the same page Dr. Burgess says: "Dr. Mason, who ultimately fell a victim to phthisis, went to Madeira with the belief that

he would recover his health under the alleged sanitary and benign influence of the climate of that island." I should like to know upon what authority Dr. Burgess makes this assertion, for Dr. Mason's editor tells us that "his visit to Madeira, and consequent residence there for a period of nearly two years, were purely accidental"; and then goes on to relate that Dr. Mason started for Nice; that "this object, however, was unfortunately frustrated" by his *disregard of self*, and devotion to a sick relative, and that he then came to Madeira; that Dr. Mason afterwards made a second attempt to reach Nice, "the climate of which, he had always been persuaded, was far better adapted to his case."

At page 13, Dr. Burgess quotes a paragraph from Dr. Mason respecting the hygrometric condition of the climate of Madeira, in which he (Dr. Mason) asserts that it is saturated with moisture during the greater part of the year, and then proceeds as follows (see page 14): "The author (Dr. Mason) supports this statement by a series of tables, from which it would appear that at the temperature of 50° , which is near the mean temperature of London, the air, if saturated, is capable of holding 100 parts of moisture in solution; while at the temperature of 68° , which is rather above the mean temperature of Funchal in Madeira, it will contain 200, or nearly double what it is able to hold in London." Judging by this paragraph, I presume that Dr. Burgess does not profess to have any knowledge of the science of meteorology; for I cannot imagine that any person at all acquainted with the rudiments of that science, could write such a sentence. First, Dr. Mason (see page 18), does *not* state that the air of London is capable of holding 100 parts of moisture, *except* upon a previous supposition, which Dr. Burgess omits, and thereby converts that statement into unintelligible nonsense. Secondly, to represent the deduction as one which appears from Dr. Mason's *series of tables* respecting the meteorology of Madeira, indicates a total misapprehension of the whole subject. Dr. Mason makes out his statement, not by his own tables, but by quoting from Mr. Daniel a physical fact in the science of meteorology. Thirdly, to suppose that Dr. Mason intended to prove, or even to support, his statement that the air of Madeira *is* saturated, by pointing out that *if it were* saturated it would contain twice as much moisture as the air of London, is too absurd. Is

this a specimen of Dr. Burgess's interpretations of meteorological observations, by virtue of which it is incontestably established that Madeira is no exception to the rule which he lays down respecting *foreign* climates? Doubtless Dr. Burgess has devoted his time to his profession, its studies and its duties; and, if he has not found leisure to make himself acquainted with the collateral science of meteorology, he is in the position of the majority of his professional brethren, and of other professional men, who, for want of time, must make some similar omissions. This may be a valid excuse for his misapprehension of Dr. Mason's statements, but hardly for putting them together in a manner which is equivalent to gravely telling us that a certain quart measure is brim full, and *supporting* that statement by reference to a *series* of tables from which it would appear that a pint measure, if full, is capable of holding 100 parts of water, whilst a quart measure will contain 200 parts, or double what a pint measure is able to hold.

Dr. Burgess, at page 14, writes, "By referring to the tables of Dr. Mason having reference to this matter, it appears that the maximum dryness observed during the leste [*the leste*] is $22^{\circ}.5$," etc. Dr. Mason's statement, see page 26, is, "By referring to the tables, it will be seen that the maximum dryness observed during a *leste*, etc." [*a leste*]. And, at page 28, obviously with reference to the same leste, or *African blast*, as Dr. Burgess would have us call it, Dr. Mason informs us that the dryness experienced during the *strongest* leste he had observed had been *equalled at Paris*. I confess I was surprised to find that Dr. Burgess had taken the trouble to examine Dr. Mason's tables, and to make deductions on his own account, but the mystery disappeared, when I discovered that the whole paragraph to which I have just referred, and which Dr. Burgess gives us as if it were his own, was, in fact, copied from Dr. Mason's book, with the exception of an insignificant (?) substitution, of *the* for *a*. We have other specimens of *insignificant* mis-quotations.

At page 15, Dr. Burgess thus writes: "The following observations with reference to the variability of the weather at Madeira, will perhaps surprise the reader: 'The very frequent and remarkable variations, in a given series of years, incontestably prove that Madeira is no more to be relied on than any other

place, for certainty of fine weather, and that it has equally its annual variations of temperature.' " Truly, the reader ought to be surprised, if he gives credit to Dr. Burgess' quotations, that such an extraordinary statement should be made by a person who had resided nearly two years in this island. But his astonishment will, perhaps, take a different direction, when he is informed that Dr. Burgess has *cut out* ten words from the *middle* of the short paragraph which he has quoted, and that those words reverse, or, at all events negative, the meaning of the paragraph as given above. Dr. Mason's statement, see page 35, is: " The very frequent and remarkable variations, in a given series of years—providing the ordinary observations of the inhabitants be strictly" [*strictly*] " correct—incontestably prove, etc." And this follows some remarks by which Dr. Mason *ridicules* the manner in which other people bring forward the testimony of the oldest inhabitants to prove, that particular seasons were variations from all former experience, and tells us, in effect, that such testimony is *not* strictly correct. Consequently, Dr. Mason does *not* assert any such fact as that which Dr. Burgess gives on his authority, and, professedly, in his words. And what Dr. Mason does say, admits of being understood to *imply* exactly the reverse. Are you surprised? I am not.

At page 16, Dr. Burgess writes, " We have already noticed that, during the prevalence of the leste, or sirocco of Madeira, the air is excessively hot and parching. Within twenty-four hours after this wind has ceased, there is a copious fall of rain; and the author [Dr. Mason] has observed a very strong precipitation of dew three hours afterwards; the thermometer being reduced from 17° to 7° of dryness on the hygrometer, and at seven the following morning, to 2°, while plants and shrubs were covered with dew. Thus we find, a few hours after the leste has ceased, the whole atmosphere, from being intensely dry, becomes surcharged with humidity." Of these three sentences, the first and last are, I believe, Dr. Burgess's, and the middle one is an extract from page 48 of Dr. Mason's book, but with the following alterations. Dr. Mason says that rain *generally* falls within twenty-four hours after the leste *has altogether ceased*, and he does not use the word *copious*. Moreover, it happens that, except on the mountains, no rain followed the

particular leste to which Dr. Burgess has thus called our attention, as will be seen by referring to the extract from Dr. Mason's journal given at page 194. And this was, moreover, the strongest leste that occurred during his residence in the island. Further, in the course of my experience, which is double that of Dr. Mason, no rain has followed any marked leste, within such a length of time as to connect the one phenomenon with the other; and I have no recollection of rain having ever so followed any other leste, though I am aware that we are sometimes told by the inhabitants that it is to be expected.

With respect to the first and third sentences, which, as I have already stated, are Dr. Burgess's, they refer to the former statement respecting *a* leste—which Dr. Burgess has chosen to write *the* leste—as if it were a fair representation of what always, or at all events usually occurs, when that wind blows; though Dr. Mason, upon whose authority these statements are made, speaks of *a* leste, and refers to a table (table xxv), in which he gives the maximum dryness of six lestes as follows:—January, 9° ; February, 9° ; March, 14° ; June, 15° ; October, $22^{\circ}5$; December, 13° . So far with regard to the fairness of Dr. Burgess's selection of statistics. Now let us see how far even they bear out his assertion, that the *whole* atmosphere, from being *intensely dry*, becomes *surcharged* with moisture. First, Mr. Glaisher tells us that, in England, the dew-point is sometimes 30° below the temperature of the air. Consequently, the fact that the wet bulb of the psychrometer was 17° , or even $22\frac{1}{2}^{\circ}$ below the temperature of the air during a leste, does not indicate a very extraordinary degree of dryness; since, if we take the temperature of the air at 80° , the corresponding depressions of the dew-point, according to Mr. Glaisher's tables, are $25\frac{1}{2}^{\circ}$ and 33° respectively. Also, the corresponding quantities of aqueous vapour in a cubic foot of air are 4.69 grains and 3.54 grains respectively; whilst the air of London, at the mean temperature of 50° , *if saturated*, would contain only 4.28 grains. Secondly—How does Dr. Burgess arrive at the conclusion that a few hours after the leste has ceased, the whole atmosphere becomes *surcharged* with moisture? Does he infer this from the fact that *dew* was precipitated, or from the fact that the so-called Mason's hygrometer never shewed a depression of less than 2° ; which, according to his

table xxxii, corresponds to $4\frac{2}{3}^{\circ}$ on the dew-point hygrometer : for Dr. Mason does not say on *the* hygrometer, but on *my* hygrometer—a variation which, to Dr. Burgess, probably appeared immaterial. As to rain generally falling after a leste, supposing it to be a fact—what then? We have in England heavy showers of rain—thunder-showers—within very much less than twenty-four hours after most oppressive heat. I must say that, so far as my experience goes, I think our total exemption here from that oppressive electric state of the air which, in England, often precedes a thunder-storm, fully compensates us for our occasional “African blasts.” Lastly, I must add what Dr. Burgess omits to tell us, namely, that during the leste selected by him as a fair example with respect to dryness, though it was the strongest ever experienced by Dr. Mason (see p. 194), the maximum temperature of the air was 81° F., which is *less* than the maximum usually attained during the summer in some parts of England.

At page 17, Dr. Burgess says, “Madeira seems to have no more immunity from disease than other places. Dr. Heineken and Dr. Gourlay both agree that no disease is more common amongst the natives than pulmonary consumption, and Dr. Mason corroborates that view.” Dr. Heineken has given an opinion which I have already quoted, that Madeira *does* enjoy much greater immunity from diseases than other places. Dr. Gourlay perhaps disagrees with Dr. Heineken upon this point, since he enumerates several causes, *peculiar to the natives*, which produce disease amongst them. How far these two physicians agree with respect to the prevalence of pulmonary consumption, I have not the means of ascertaining; but the following extract from Dr. Gourlay, page 90, puts his opinion in a somewhat different light. After referring to this island as the favourite retreat of consumptive patients from the northern parts of Europe, he adds: “Yet still, though so highly beneficial in this disease, with the natives of other countries, it is not to be concealed, that no malady is more prevalent here than phthisis with the natives of the island.” Why did not Dr. Burgess quote Dr. Gourlay’s opinion whole and entire, as he expresses it? He might have added, also, some extracts from the 5th chapter of Dr. Gourlay’s book, which enumerates some of the reasons

(which might be greatly increased in number) why the natives are subject to disease—reasons which in no way apply to invalid visitors. Dr. Mason probably knew very little about the matter; and he only ventures to say, that he *should be inclined* to corroborate Dr. Gourlay's opinion, that consumption and scrofula are frequent in Madeira: which means, only, that he would do so if he could.

I must now apologise for some portions of this very long letter. In the course of writing it, I have been somewhat drawn aside from my original purpose, and I must now state that I by no means wish my numerous criticisms of one writer in particular to be taken as having any general application, or as indicating my opinion of any other medico-meteorological writer whatever. I ought, no doubt, to revise what I have written, and render it more strictly consistent in its several parts; but you know that I am on the eve of a voyage to England, after a very long absence, and cannot possibly find time to do so. For the same reason, I am compelled to omit some remarks which I intended to make on the great and peculiar difficulties of making meteorological observations in this island as compared with England—partly with a view to point out how much must depend upon the judgment and discretion of the observer.

I shall conclude with one or two specimens of reasoning, which appear to me to be more than commonly loose.

Of what value is the argument that, in the course of a few generations, the human system becomes acclimated, and therefore, that the climate in which an individual and his ancestors have been born and lived, must be the best suited to that individual? Admitting this to be true as a general rule, surely persons who are afflicted with organic disease *may be* the exceptions, and, whether they are or not, should be determined by observation not by theory.

Dr. Burgess thinks that it is inconsistent with the laws and operations of nature that the country in which an individual was born, reared, and previously enjoyed good health, should be *no longer* suited for him when afflicted with organic disease. I suppose we must yield to Dr. Burgess what he appears to assume as a point too clear for dispute, namely, that the country in which an individual was born, reared, and enjoyed good health *down*

to a certain period, and then became afflicted with organic disease, is thereby proved to have been, of all climates in the world, the one which was best suited to his constitution *down to that period*. Still I cannot help thinking that another person who adopts as an *a priori* theory, that different climates may not improbably be suited to the same individual in two totally different states of his bodily health, proves himself to be possessed of an equally clear insight into the laws and operations of nature. Secondly, I should like to be informed whether it can fairly be predicated of the generality of persons who are afflicted with organic disease, that they were born and *reared* in the enjoyment of good health, and totally unaffected by disease down to the period which the word *reared* may imply. Thirdly, if that is to be answered in the affirmative, are there not, at least, a great number of persons who are afflicted with organic disease from their birth, or during infancy, or at all events before they reach the stage of life to which Dr. Burgess refers? Fourthly, would it not tend to clearer conceptions of the matter, if such statements as the one which I have last quoted, were made in somewhat more logical form, so that we might see, without reading every sentence two or three times over, what are the premises assumed as the foundation of the argument, and understand the nature, value, and extent of the conclusion. I feel sure that no one who, placing some degree of confidence in the author, reads such an argument only once, and with ordinary attention, can have a just notion of the course of reasoning through which he is led, as it were blindfold, and consequently at the risk of being misled. Fifthly, since the author in question thinks, as he tells us elsewhere, that the laws and operations of nature do not render it necessary for an invalid to be confined to the place in which he was born and reared, but that a proper locality should be selected within the limits of his own country, he ought to state whether England, Scotland, and Wales, are to be considered as one or as several. We understand that it is distinctly prohibited to an invalid native of Dover to cross over to Calais or Boulogne, and *vice versa*; but we are left somewhat in the dark whether the Tweed forms a boundary line in Dr. Burgess's medical map of climates. Apparently the charm lies in the adjective *foreign* climate; but in a matter of so much importance to the public, the author ought to be more explicit.

With respect to Madeira, however, we have no reason to complain of want of explicitness. It is sufficiently obvious that Dr. Burgess regards it, in comparison with England, as an *extreme* climate. But upon what grounds? We may presume that he never placed himself within reach of the "African blast", and consequently that all his information is drawn from other sources than personal knowledge or experience. Respecting the manner in which he interprets, understands, and represents those authors whom he cites as authorities, and from whom, it is fair to presume, he has derived the principal part, if not the whole, of his information, I need make no further remark. I hope and believe that I have furnished you (not so much for your information as for your use) with better, fairer, and safer means of forming your own opinions of the merits, and of the reliance to be placed on his book, so far as regards the climate of Madeira, than I should have done if I had followed his example, and merely given you, under the name of a summary, a few extracts from what he has written on that subject, taking to myself, of course, as much liberty of interpretation and representation as he has thought himself justified in making use of with reference to other authors.

Whatever benefits Madeira may confer on invalids, in the shape of change of climate, it is somewhat remarkable that most authors, except Dr. Burgess, dwell a good deal on its mildness, and speak of the benefit it confers by enabling invalids to *avoid* the extreme changes of our English seasons; or, as Dr. Gourlay expresses it, "to cheat the winter of their own climate". If, however, people imagine that the valley of Funchal is a hot-house or a green-house, where even an eddy or current of air is not permitted to ruffle the leaves of a tender plant, they greatly deceive themselves; for, though we never have those severe, cold winds to which many parts of the Continent are liable, yet moderate breezes, amply sufficient to renovate and purify the air, are by no means uncommon.

Believe me to be yours most sincerely,

J. M. BLOXAM.

GEORGE LUND, ESQ., M.D.